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STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF CHICAGO.

COMMUNICATED BY PROFESSOR JAMES ROWLAND ANGELL.

### IV. PSYCHO-PHYSICAL TESTS OF NORMAL AND ABNORMAL CHILDREN. — A COMPARATIVE STUDY.

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The tests reported below were made upon the pupils of the elementary school of the University of Chicago, and the cases in the Chicago Physiological School. The pupils in the first school are normal children, between the ages of four and fourteen, influenced by hereditary and environmental conditions probably superior to those of the average child. The cases in the Physiological School are backward and defective children who have failed to show the necessary ability to make satisfactory progress in the public schools. Some of these children rank quite low in intelligence but all are believed to be educable. They range in age from nine to nineteen years. The physician's examination frequently, in fact usually, brings to light quite unfavorable hereditary influences. There are also numerous and sometimes pronounced stigmata of degeneracy present. In some cases the defects are due to arrested development, either pre- or post-natal. The present study, however, is devoted to the existing conditions and possible progress of these cases under such favorable surroundings and pedagogical procedure as the Physiological School affords.

## THE NATURE OF THE TESTS.

The tests covered rather a wide range. This is a necessary incident of pioneer work. Very few tests were made, however, which did not yield readily to consistent interpretation. Despite their number the tests may be divided roughly into three classes. There were the ordinary tests of the senses of hearing, sight, taste, smell, touch and temperature together with sensitiveness to pain. There was a series of muscular tests, involving numerous forms of motor coördination with special reference to rapidity, accuracy and steadiness, of movement, and fatigue. And third, a number of tests were made with special reference to prevalent forms of imagery in peculiar types of children, certain emotional reactions, etc. Effort was made to conduct the experimentation under approximately identical conditions of time, place, environment, fatigue and so forth. At the Elementary School there was taken simultaneously a series of physical measurements, including weight, height, lung capacity, strength of grip, and the numerous girth, depth and breadth measurements.

These measurements, however, are not reported in this paper. At both schools the record of each child was carefully preserved for comparison and reference.

## THE PURPOSE OF THE TESTS.

Certain definitely formulated purposes were held in view throughout this work which extended over part of one school year and all of the next.

(a) It was deemed desirable to get psychological data which would serve in determining the most fruitful pedagogical procedure for each child. This purpose proved immediately practical in numerous cases.

(b) It was believed by fashioning the tests to the possibilities of the two widely divergent classes and making them as nearly identical as possible, some ready and simple method might be determined of differentiating the abnormal from the normal child. Observation shows that about one child in fifty in our public schools is utterly incapacitated by physical defect or mental incapacity from reacting spontaneously to certain valuable parts of his environment.

(c) Furthermore, it was believed that some light might be obtained upon the problem of finding a substitute for the present formal, arbitrary and confessedly inadequate methods of determining children's real mental capacity.

(d) And finally and chiefly, it was hoped that some contribution might be made to the present meager knowledge of the psychical life of the child in each of the two disparate fields of investigation. The author of the present paper makes no claim to have accomplished much as yet in the realization of the last-named purpose. In fact, the work here reported has been done with the belief that such knowledge will not be secured by the promiscuous massing together of data obtained from different sources, by different individuals of varying ability in observation and insight, nor by one well-trained individual selecting his data from the same sources, but who, ignoring the necessity of following up the children in their slow development, fails to adopt the principle of repetition in his experiments. Most of the tests here reported have been repeated one or more times and it is with the expectation that the process of repetition will be continued that the work is now made public.

#### THE SENSORY TESTS WITH THE NORMAL CHILDREN.

An astonishing thing about the ordinary sense tests is the fact that although no great amount of skill or expenditure of means is required for their prosecution, teachers and parents remain ignorant of readily detectible and usually remediable defects. These defects, moreover, are continually interfering with the progress of the child if not indeed threatening the existence of much of the valuable content of his life.<sup>1</sup>

*Hearing and Sight.*—Of 53 pupils in the Elementary School whose hearing was tested, 3 were found seriously defective. But 1 pupil was found lacking in keenness of vision. 61 per cent. of the pupils tested were astigmatic, though of this number but 2 or 3 required immediate medical attention. Out of a total of 66 there were 27 cases of slight color blindness, 2 cases being so pronounced as completely to incapacitate the pupil for certain

<sup>1</sup>Rowe's 'The Physical Nature of the Child' is the briefest and most concise statement of practical technique in this series of tests.

kinds of school requirements. 11 of these matched the standard colors but were confused on shades, 13 were confused on greens and blues, though most of these could distinguish between the standard blue and green. 4 confused green and yellow shades, 2 pink and blue, and 1 green, blue, and yellow.

#### TASTE AND SMELL.

The facts as to sensitivity in taste and smell are illustrated in Tables I. and II. Table I. shows the threshold of sensitivity

TABLE I.

TASTE AND SMELL, ELEMENTARY SCHOOL; THRESHOLD.

		Taste.				Smell.	
		Quinine.	H <sub>2</sub> SO <sub>4</sub>	Saccharine.	Salt.	Violet.	Cloves.
<i>a</i>	Boys.	4	2	3	3	7	2
<i>b</i>		16	15	18	16	8	4
<i>c</i>		6	8	6	8	16	22
<i>d</i>		1	1	0	0	0	0
Total		27	26	27	27	31	28
<i>a</i>	Girls.	1	2	2	0	7	0
<i>b</i>		13	11	11	13	3	3
<i>c</i>		2	3	3	3	6	13
<i>d</i>		0	0	0	0	0	0
Total		16	16	16	16	16	16

TABLE II.

TASTE AND SMELL, ELEMENTARY SCHOOL; DISCRIMINATION.

		Taste.				Smell.	
		Quinine.	H <sub>2</sub> SO <sub>4</sub>	Saccharine.	Salt.	Violet.	Cloves.
Named.	Boys.	25	12	27	27	28	7
Not Named.		6	19	4	4	1	16
Total.		31	31	31	31	29	23
Named.	Girls.	15	15	15	16	19	4
Not Named.		0	0	0	0	0	10
Total.		15	15	15	16	19	14

as indicated by the naming of the sensation quality. While this ability to command the word corresponding to a sensation quality may not be a strictly reliable criterion as to the point



where the actual discrimination occurs, it has in its favor the fact that it is sufficient for all practical purposes. Besides with children it is quite difficult for the observer to hit upon any more reliable criterion for the determination of the discrimination limen. That the actual discrimination sometimes occurs before the required linguistic capacity appears, is not to be questioned.

In Table I. the letters *a*, *b*, *c*, *d* refer to the different strengths of solution. For quinine they are respectively:

.00004 per cent.; .0004 per cent.; .001 per cent., and .0021 per cent.

For Sulphuric Acid — .001 per cent.; .008 per cent.; .016 per cent., and .024 per cent.

For Saccharine — .0005 per cent.; .0025 per cent.; .025 per cent., and .05 per cent.

For Salt — .01 per cent.; .13 per cent.; 2 per cent., and 2.8 per cent.

The smell solutions consisted of extract of violet: .0000001 per cent.; .00001 per cent.; .1 per cent., and .5 per cent.; and oil of cloves, .0000001 per cent.; .00001 per cent.; .001 per cent., and .004 per cent.

The first in each series is below and the fourth is above the ordinary threshold; the second and third are within the range of the normal threshold. The girls have a uniformly lower taste threshold than the boys.

The results in smell are interesting though the significance is sociological rather than psychological. Half of the boys failed to detect the violet until the third solution was reached, while almost half of the girls detected it in the first solution. There is a general deficiency in detecting cloves, but the difference between violet and cloves is much more marked in the case of the girls than the boys. While nearly half of the girls detected the weakest solution of violet, four fifths of them failed on cloves until the third solution was reached. Since cooking is a part of the course of study in the Elementary School, it seems a little startling that the æsthetic odor entirely out-distances the practical one in the race for recognition.

In the discrimination tests, as was said, the sensation qualities were named. It was not required, however, that the article

producing the quality be named. For instance, 36 children said 'bitter' to 4 saying 'quinine,' and 21 said 'sour' to 2 'lemon' and 1 'puckering.' All said 'sweet' and all said 'salt.' In the first smell test 35 answered 'perfumery,' the other 12 answering 'violet,' 'cologne,' 'rosewater,' etc. Cloves afforded the greatest perplexity. Less than one third gave the correct answer and the other answers included cinnamon, peppermint, pepper, birch-bark, lead-pencil, spice and paste. Of the tastes sour is the least readily detected and named, particularly with the boys, three fifths of whom failed entirely in naming it. The transition of an agreeable into a disagreeable sensation was frequently illustrated in the case of the strong sweet solution. In 24 cases it was noticed that the fourth sweet solution was called 'bitter,' 'sour,' 'bitter and sweet,' etc., accompanied by expressions of the disagreeableness of sensation. In three cases this result was obtained from the third solution.

#### THE SENSORY TESTS WITH THE ABNORMAL CHILDREN.

A great deal of skill and patience are required for experimentation with the backward children. They must be sufficiently acquainted with the experimenter to have the 'at home' feeling. One will be suspicious of the apparatus and will even refuse to coöperate in the suggested 'play.' Others will be overwhelmed with curiosity and inquisitiveness. One who has evidently been maltreated by teachers will unconsciously place the experimenter in this 'enemy class' and will assume accordingly the attitude, if not the insignia, of a combatant. Another will consider that he is dealing with a 'doctor,' and the difficulty of getting a pure response will be chiefly due to the unconscious prejudice. John, one of the most alert cases, displayed his insight into the situation by asking the question, "You're going to see what kind of a man I will be, are you?" The different linguistic capacities of the children also introduce an element of ambiguity into the meaning of many of the responses, and the experimenter needs to understand not only each child's vocabulary but the peculiar meaning which each word conveys to his mind. In short he ought as much as possible *to live with the children*. The children reported upon in this paper ranged in age from ten to twenty-two.

*Color.*—The appreciation of color is certainly not so highly developed as is sometimes thought. Of twelve carefully tested six showed unmistakable evidences of color blindness, while of the total number only two (both girls) possessed anything like an accurate color vocabulary. There were four distinct cases of green-blue blindness. One, a very backward child, who was not deemed sufficiently educable to remain in the school, showed no evidences of any color appreciation, though very persistent efforts were made to elicit such. She sorted the yarns without a mistake on the basis of their form and this in the face of the fact that their form-difference was so slight that the work was almost completed before the experimenter himself detected what was going on. Another showed after repeated tests of different kinds covering the entire school year, that her ability at color discrimination was practically limited to yellow and red. In sorting colored beads great readiness was shown by her in discriminating yellow from red while there was utter confusion on blues and greens. Beside the above-mentioned color names, brown, purple, and pink figured prominently in the color vocabularies of these children.

*Color Threshold.*—An experiment was devised to determine the color threshold. Color disks were arranged on the wall and the child led so far away as to be unable to detect any color. He was then led up slowly until the colors appeared in succession. The impossibility of maintaining a uniform degree of illumination, together with numerous other drawbacks in the matter of technique makes the absolute distance here of little value, but the relative results are believed to give an accurate expression of the situation. The experiment was conducted satisfactorily with seven of the cases. With each of these, red appeared first both in the first series of tests and in the second, which was taken some months later. In the first series green came second in four cases, blue being second in the other three cases. Green was third in one case and yellow in six cases. Without further particularization, the order was *red, green, blue, yellow*. When the series of tests was repeated some months later, red still uniformly held first place but there was a general disposition to move yellow forward, the other colors,

except in one instance, maintaining the same order as before. This change in yellow was due partly to the intervening color education and partly to the fact that the child had learned that when he saw no color the color was probably yellow. Experiments of this kind are absolutely valueless, unless the experimenter is intimately acquainted with the vocabulary of the subject.

*Color Preference.*—An effect was made to get at the children's color preferences. Many of the results were conflicting, but five had a distinct preference for red. One each preferred indigo, pink, and green. There were a few marked cases of emotional accompaniment in this color preference. Ralph called red 'my color' and told with much glee of a red, white and blue flag which he had at home. J. liked indigo best and had an indigo ribbon in her hair. Frances liked pink best and her room at home was furnished in pink. John did not like red for he was once 'scared almost to death by a red cow.' The persistence of this prejudice was shown in the fact that the same circumstantial evidence against red was produced by him four months later than the first test. E. — a cretinoid — seems to partake of the ancient Greek conception of the beautiful. On both occasions — months apart — when the color preference question was put to him he carefully arranged the yarns in symmetrical rows, but did not seem to care to commit himself to any one color. The 'pretty' or 'like best' idea seemed to be better expressed in symmetry, form and order than in color. Other evidences of this peculiarity will be cited under another head.

*Hearing.*— But three cases in the Physiological School had perfect hearing in so far as this could be determined by the ordinary watch tests. In most of them both ears were affected.

*Pitch.*— The Galton whistle tests indicate a poor ability at pitch discrimination also, the upper limit never being above 28,000, which was rare, and running down as low as 14,000 vibrations.

*Sight.*— The eyesight of these abnormal children is poor; about half of those tested were below the standard in keenness of vision, and some degree of astigmatism was found in every child but one or two.

*Taste and Smell.*—It was usually impossible to distinguish between the threshold and the discrimination point. Three cases were entirely lacking in the sense of smell, no distinction being recognized between the strongest solution of violet and cloves. Four were more or less deficient in taste, while the average threshold is much higher on all than in the normal children. Nearly 80 per cent. of the children tested recognized nothing below the third strength of quinine solution.<sup>1</sup> Nearly half failed entirely with sour, 65 per cent. failed on sweet, and 63 per cent. on sour until the third solution. Their detection of violet perfume was almost as accurate as with the normal children, but one only detected the first solution of cloves and no other one detected the second solution. El. called all strengths of sour solution 'water.' R. called salt 'sweet' and sour 'bitter,' and while he called quinine 'bitter,' he applied the same name to each of the other tastes, and gave no indications of especial displeasure at the strongest quinine solution. When asked, he said he didn't like it. Em. uniformly called sugar 'bitter' and sour 'sugar.' Similar phenomena have been observed by Wylie in the laboratory of the Minnesota School for Feeble-Minded.<sup>2</sup>

*Pain.*—On the right temple the algometric readings range in kilos from 1.01 to 4, half of them being above 3.25.

*Temperature.*—*Discrimination.*—One case was able to discriminate differences as low as 5° C.; two as low as 1° C., while the rest ranged from 2° C. to 10° C. This power of discrimination tallied very closely with the general intelligence of the cases. The standard from which measurements were taken was about 30° C.

The threshold of the sensation 'warm' was located on the C. scale at a region ranging from 18° to 32° that of 'hot' from 30° to 55°, and that of pain from 49° to 65°.

*Fatigue.*—Dynamometric readings range with the right hand from 5 to 63, all but the lowest cases in intelligence being above 24, while nearly all were above 30. With the left hand the readings were almost uniformly a little lower (except in

<sup>1</sup> See Tables I. and II.

<sup>2</sup> *Journal of Psycho-Asthenics*, p. 109, March, 1900.



case of the left-handed subjects). The dynamometer readings were taken after the day's work and are considerably smaller than readings obtained on days when no gymnasium work was given. This emphasizes the fact brought to light on all sides, that fatigue with backward children, as would be expected from their low vitality, is very rapid and considerable.

A little experiment was made with four cases with the double purpose of testing this fatigue and also the effect of summation of stimuli under fatigue. Fifteen successive trials were given with dynamometer in each hand. The average of each five tests are reported. During the last five the experimenter, simultaneously with the subject, went through the customary movements accompanying the gripping process. With some cases lower in intelligence than here cited, the additional energy was expended but it took the form of divers facial grimaces and other contortions not measurable with the dynamometer. The following results are of interest: Ralph: R, 29, 24, 28; L, 23.5, 17, 18. John: R, 61, 56, 58; L, 60, 58, 53. Tom: R, 30, 25.8, 26.4; L, 25, 24, 23. Joe: R, 37.6, 36.6, 34.4; L, 20.4, 19.4, 20.8. (Joe is left-handed.<sup>1</sup>)

*Touch.* — An effort was made, but without very satisfactory results, to obtain information as to the sensitivity of the tactual, pressure and muscular senses. Two serious difficulties lie in the way here. The process is tedious under the most favorable conditions and the abnormal child does not possess the delicacy of attention necessary to insure reliability in results. The fatigue element is also a serious interference. Beside this the child soon lapses into a series of rhythmic responses the stress of which prevents successful procedure. By means of the method of right and wrong cases, however, some work was done with E. which is considered approximately correct.

The æsthesiometric compass reading on the forefinger was 10 mm., while there seemed to be but one touch area on the little finger. The reading on the palm was 41 mm., wrist 10.5 to 11 mm. and forearm 55 to 60 mm.

<sup>1</sup> The ordinary dynamometer is wholly unfit for tests with children. Mr. F. W. Smedley, of the Chicago public schools, is using an excellent dynamometer constructed for use with children.



The average error in locating spots touched on the wrist was 15 mm. with the prevailing tendency 'in' and 'up.' Spots touched on the fingers were located quite accurately. No finer discrimination was made in active weight than that between 100 g. and 60 g.

*Imagery.* — A great deal of time and patience were expended in the effort to determine the prevailing form of imagery in each of the abnormal children. Table III. contains some of the re-

TABLE III.  
IMAGERY OF ABNORMAL CHILDREN.

Case.	Age.		Ear.	Eye.	Ear and Voice.	Eye and Hand.	All.
E.	19	4	27 20, 4	42 20, 2	56 20, 1	37 0, 15	74, 13
J.	13	4	80 20, 5	82 20, 5	72 20, 9	80 1, 4	80, 20
F.	14	5	80 20, 2	80 20, 2	72 20, 1	80 20, 2	
El.	12	5	55 20, 3	42 20, 2	60 20, 1	30 1, 5	
Em.	14	5	80 20, 2	57 20, 4	52 20, 5	73 2, 7	57, 8, 44, 11
R.	9	5	80 20, 1	80 20, 2	80 20, 2	80 20, 2	
M.	13		80 20, 1	72 20, 2			
Ju.	12	6	80 20, 2	72 20, 0		(?)	
T.	9	4	86 20, 15	87 20, 15	61 20, 11	24 2, 8	

sults obtained. The first column shows the highest number of nonsense numerals the child could repeat immediately upon their being given him *orally*. It was found that the number varies somewhat according to the character of the original impression. It was also found that the length of time such a series can be remembered depends upon the same condition. Some can do better, both as regards number and time, with a spoken series, others with a written series and so forth.<sup>1</sup> This suggests an important shortcoming in the ordinary method of procedure here; it at least puts an important limitation on the meaning of the responses obtained by the ordinary procedure, and this limitation has, of course, greater applicability to abnormal than normal individuals.

As it was found when numerals were memorized that no case 'broke down' under four numerals, that number was chosen and uniformly used in subsequent experimentation. Five series of tests were made. In the first, four numerals were repeated orally three times. After ten seconds the subject repeated the

<sup>1</sup> Cf. G. Whitehead, *PSYCHOL. REV.*, 1896, p. 258.

series. In another ten seconds he was asked to do the same and so on, until he had had five trials at this series. This made a total of twenty possible numerals and five series. This process was repeated with a new set of numerals over and over, until the subject had a chance at a possible eighty numerals and twenty series.

The numerator of the first fraction indicates the number of numerals correctly given out of a possible eighty. The numerator of the second fraction is the number of correct series out of a possible twenty. In the second test the same *modus operandi* exactly was followed except that the numerals were *written* and the subject silently looked at them ten seconds. In the third test the subject *heard* and himself *pronounced* the numerals and then waited the customary ten seconds. In the fourth, the subject himself *copied* the numerals after which the paper was removed. In the last test all methods of stimulation were combined. The prevailing types of imagery in each subject are apparent from the table. Some of the results are quite pronounced and are verified by other forms of tests. T.'s results are not complete, but there is cumulative evidence that he is a visualizer. Three cases are perfect in the auditory form. R. does best in the vocal-motor form. It was found that the results vary considerably according as the responses were given *orally* or in *written* form. This phenomenon was so pronounced in Em.'s case — a child with a marked speech defect — that some special results are given in her 'all' column. The first fractions are her oral answers and the second fractions are her written answers on identically the same series. That is to say, she would be confused in her oral answer but would immediately proceed to write not only the numbers but the series with absolute correctness. Moreover, she was utterly unconscious of the disparity in the results thus successively given in the two distinct ways. This test was repeated some months later and the same phenomenon occurred. This case seems to be similar to one reported by Charcot, and will doubtless yield to neurological interpretation. The work showed the familiar result that in the process of breaking down of memory there occur in order: (1) a change of *order* of the numerals, the same numerals being

present in the response, (2) *the dropping out* of certain ones and the *inserting* of others, (3) the *dropping out*, decreasing the number and (4) the *adding* of extra numerals. The evidences of the operation of rhythm appear not only in this statement, but are constantly cropping out in the daily work of the children.

#### MOTOR COÖRDINATION.

Table IV. contains some results in motor coördination, accuracy and rapidity. The coördination tests consisted in sorting 100 beads into four boxes on the basis of color, there being 25 each of red, yellow, blue and green beads. The beads were put into a bag and one was withdrawn at a time, the time to complete the entire work being accurately recorded. The bag was held by one hand while the sorting was done with the other. This color test was followed by a sorting test of 100 forms of the same color under like conditions. The forms consisted of cubes, cylinders, spheres and parallelepipeds. In both cases the number of mistakes made was counted, although as this was not done with all the children the results are suggestive rather than scientifically valuable. This feature should be more carefully attended to in future tests.

*Accuracy.* — The tests in accuracy were made upon the first joint of the forefinger and upon the axial joint. A straw twelve inches long was attached to the finger, all the other joints of the finger being stiffened except the first joint, and with eyes closed the child made the 'least possible movement.' A light fishing pole 79 inches in length was under similar conditions attached to the arm and the least perceptible movement was made from the shoulder. The subject was not allowed to grasp the pole with the hand. Three trials were given in the case of each test and the average computed. In the table these results are expressed in terms of angular measurement. It would be well in subsequent tests carefully to keep the results of the three trials *in their order*, as it is believed some significant truth would come to light here. It is by no means true that the first trial shows the lowest degree of accuracy as might be expected.

*Rapidity.* — In the rapidity tests a fatigue counter was used. The subject being in a comfortable position, his arm was fastened

TABLE IV.  
MOTOR COÖRDINATION.

A.	Group.	Color.			Form.			Accuracy.			Rapidity.		
		No.	R.	No.	L.	No.	R.	No.	L.	No.	Finger.	No.	Arm.
	Kindergarten.												
	III.	5	443	4	393	4	346.25	2	349	4	2° 13'	3	1° 9' 33"
	IV.	12	320	8	333	7	300.3	7	285	7	2° 55' 47"	7	1° 9' 02"
	V.	13	293	10	289.6	10	255.6	10	262.8	11	1° 47' 35"	10	38' 52"
	VI.	5	295	1	283	1	232	1	233	1	2° 37' 47"	2	1° 9' 02"
	VII.	5	294							2	2° 26' 30"	2	1° 26' 20"
	VIII.	14	224	9	229	9	188.5	8	195	15	1° 47' 04"	15	22' 45"
	IX.	7	219	3	215	3	204	3	178.3	5	1° 13' 16"	5	22' 47"
	X.	7	190	8	211.3	8	180.6	8	178	16	0° 54'	16	19' 31"
		9	184	9	181	6	158.8	6	126	8	0° 44'	8	16' 26"
B	Total.	83	263.5	52	265.7	48	247.5	45	224.6	73	1° 31'	68	36' 37"
C		44	254.4	44	259.1	44	225.3	44	224.1	72	26	73	30

To determine the average age of the children in each group add three to the number designating the group.

to the table in such a way as to allow free movement of the forefinger only. He then manipulated the counter with the utmost rapidity possible. The test continued for 60 seconds, the readings being taken at the end of each ten seconds. The number in each ten-second interval was then computed and the average taken for the six intervals. It is this result which is found in the table, column 'Finger.' The other 'rapidity test' was similar to this except that the shoulder joint moved, the arm and wrist being stiffened. From these same results the subject's fatigue curve was computed and the results preserved on his card for future reference. By comparing the number marked by the counter during the first ten-second interval with the number marked during the sixth interval the rapidity of fatigue comes to light and is subject to mathematical statement. Some of these results are made use of later on, and this should be made a prominent feature of subsequent work.

Table V. gives corresponding results for the cases of the Physiological School. In seven instances the tests were repeated after an intermission of about three months. The results of the second trial follow in the table, immediately after the first. The columns marked 'm' contain the number of mistakes made. In the color test for the first trial 100 colors were used, 25 of each of those mentioned in connection with Table IV. Unfortunately, however, but 75 forms were used at first, 25 of each kind. Later, when the value of this work had become manifest, the experiment was reduced to a uniform system and 80 colors and forms respectively were used. This number was chosen, as it was found that 100 makes too great a tax upon the attention of backward children. Each series of results is, therefore, inherently valuable, but the series are not subject to reliable comparison among themselves. The 'accuracy' and 'rapidity' sections in Table V. are constructed in the same way as the corresponding sections in Table IV. Under accuracy, however, it will be observed that two columns are given to the finger and two to the arm. It was found that these children could only understand what was required of them in this test by first allowing them to perform the work with the *eyes open*. The second column in each case gives the results taken immediately after-

TABLE V.  
MOTOR COORDINATION. PHYSIOLOGICAL SCHOOL.

Case.	Color.				Form.				Cards.		Accuracy.				Rapidity.		Fatigue.	
	R.	M.	L.	M.	R.	M.	L.	M.	Colors.	Pictures.	F-o.	F-cl.	A-o.	A-cl.	F.	A.	F.	A.
E.	360	1			230	5			127	130	2° 5' 14"	3° 34' 35"	0° 30'	2° 37' 37"	35	29	75%	Inc.
R. S.	358	2	366	0	372	1	290	2	152	187	1° 47' 22"	5° 3' 35"	1° 13' 24"		21	19	66	75
T.	360	14			225	11			(?)	160	2° 5' 14"	5° 3' 35"	1° 16' 8"	3° 48'	21	23	36	84
T.	244	1	340	11	270	11	237	5							26	20	31	40
Jn.	250	1	310	1	221	3	207	1	105	125	1° 29' 30"	1° 47' 22"	1° 21' 33"	2° 43'	28	26	66	77
Jn.	270	1	275	1	237	2	207	0	82	101	3° 16' 43"	5° 21' 20"	1° 2' 31"	4° 4' 20"	30	20	87	76
J.	320	430			250				178	180	2° 41'	17° 53' 45"	1° 59' 35"	4° 45'	28	22	51	54
J.	352	9	325	1	282	1	274	1	145	152	1° 29' 30"	5° 56' 47"	1° 27'	1° 51'	22	25	Inc.	90
J.	345	11	488	11	223	1	240	12	85	100	2° 41'	7° 42' 41"	2° 15' 53"	4° 9' 45"	23	22	81	91
F.	332	4	325	4	230	0	260	0	102	97	1° 29' 30"	2° 58' 50"	1° 51' 27"	2° 37' 37"	25	22	34	71
R. N.	380	2	410		200				73	60	2° 23' 8"	9° 27' 43"	1° 29' 42"	1° 29' 42"	21	24	72	81
R. N.	266	1	252	2	216	2	230	2	55	102	1° 47' 22"	2° 23' 8"	0° 49'	2° 30' 30"	20	23	70	Inc.
El.	600	16	480	28	300	2	210	4	(?)	160	7° 7' 30"	14° 3' 10"	2° 25' 26"	5° 47'	14	9	Inc.	Inc.
El.	455	many	450	25	275		263	0	120	115	4° 45' 44"	8° 0' 14"	2° 15' 53"	2° 54'	19	14	46	83
Em.	400	5							180	215	2° 23' 8"	3° 34' 35"	1° 24' 16"	1° 29' 42"	10	22	Inc.	Inc.
Em.	450	6	430	2	383	3	380	3	160		2° 23' 8"	4° 28'	1° 24' 16"	1° 51'	10	19	(?)	(?)
1st av.	379		425		236		223		112	125					22	21		
2d av. <sup>1</sup>	335		345		268		263		109	113					21½	20½		

<sup>1</sup> J being left-handed is omitted from the average.

1st trial = 100 colors, 25 each. 1st trial = 75 forms, 3 kinds.

40 of each kind of cards.

2d trial = 80 colors, 20 each. 2d trial = 80 forms, 4 kinds.



ward with the eyes closed. The 'Fatigue' column in this table gives in per cents. the value of the numbers recorded by the counter during the last ten seconds, in terms of the number recorded by the counter during the first ten seconds. The letters 'inc.' mean that in the instance cited the last number was larger than the first number. This does not mean so much an absence of fatigue, as an increase in skill in the required manipulation.

Tests were also given with three sets of cards. There were forty cards in each set. In one set, ten each of blue, red, green and yellow disks had been pasted on the cards. In another set there were printed ten each of four familiar pictures, *i. e.*, a boy, a hand, an ox-head and an eye. In the third set there were printed on ten cards a triangle, on ten a square, on ten a parallelogram and on ten a circle. The columns of the table contain the number of seconds required to sort the different sets. The card tests, though they have been much used, are essentially misleading. They have their value, but it is not that usually attributed to them. They afford a crude means of determining reaction time when the afferent current is visual. They also indicate a child's relative appreciation at a given moment for pictures, colors and forms, which is of pedagogical interest. But it is unpsychological to suppose that they offer any reliable comparison between color and form perception, since they entirely ignore all elements of form appreciation except that by means of vision!

We call attention first to Table IV., *B*. Here the results are given in totals. The actual results here are not considered of importance, for all the experiments of a given kind are dumped together regardless of the age or advancement of the children. But the results in the larger view, as gathered from Table IV. and Table V., have this importance, that they show in every case the abnormal children take more time for coördination, are less rapid and accurate in movement, and more rapid in fatigue. Both normal and abnormal children agree in taking *more time for color coördination than form coördination* — that is when the beads are used, and as for the cards they offer no test of the tactual sense so invaluable in form appreciation.

There is a vast difference between feeling and seeing forms. The chief value of the card results, so far as they throw light upon color and form appreciation, is this negative consideration. Both classes of children agree also in *more rapid form discrimination with left hand* and *more rapid color discrimination when the right hand is used*. This will be referred to again. It will also be seen that both classes of children are *more accurate with the large than with the small joint*. With the abnormal children the finger joint has a fractional advantage in the rapidity test, but as the apparatus for testing the axial joint was somewhat complex, these children had difficulty in keeping it going. The lower average is probably due to this fact.

In Table IV. the normal children are arranged on the basis of their standing expressed in terms of groups in the Elementary School. From each of the four columns showing coördination times it will be seen that there is a steady and uninterrupted decrease in the time of coördination from the kindergarten to the tenth group. The only exceptions to this statement are found in the fifth and sixth groups, color, right hand, but it will be observed here (*a*) that the number tested in these two groups is quite small and (*b*) even in spite of these facts, if the results of one pupil were taken out in each case, the totals then obtained would swing readily into line. The well-demonstrated fact must not be overlooked also, that children of this age are undergoing certain very definite and rapid physiological changes. The general principle stated above applies, though not with such absolute uniformity, in the cases of accuracy and rapidity. The fact much emphasized in recent writings is also worthy of notice—and this is particularly important educationally speaking—that children are *more accurate in their movements and move with greater rapidity when using the shoulder joint than the finger joint*. This suggests the necessity of what is already becoming recognized in some places, the necessity of a complete readjustment of kindergarten methods to the freer and larger movements for which the child is physiologically prepared. The setting of children to tasks requiring fine finger movements and delicate discrimination is certainly a species of malpractice. The figures also show that as children increase

in wisdom and stature they become comparatively more dexterous with the finger, although even in the tenth group the arm has the absolute advantage.

In the course of such experiments as the above, the experimenter has over and over again many opportunities for getting a practical picture of the child's mental alertness and mental habits as well as his ethical motives. A fruitful field in which the latter crops out is found when the child picks up two beads, after having been strictly enjoined to take but one at a time. The numerous alternatives taken in this particular crisis are very significant indeed. A comparison of the coördination time with the right and that of the left hand disclosed some interesting results. A hint at the question is obtained from Table IV., *B*, and again from Table V., '2d av.' In both cases it appears that the longest time for sorting is with colors and the left hand. The colors right hand comes next, and quite below *these* figures come in order the forms right hand and finally for the shortest time the forms left hand. When it is remembered that Table IV., includes all ages and that only about one half of the normal children were given each of the four tests it may be concluded that the results have no significance. The average, made up from data representing such children as those reported in Table V., if taken alone, would certainly be no trustworthy guide to scientific truth. To be sure of the situation the results for 44 right-handed children are given in Table IV., *C*. Each of these 44 children took the four tests. The *relative numbers* are the same as shown in the tables just cited. Of the 44 cases 20 actually sorted the forms sooner with the left hand and the margin was close in some of the other cases. Of the same 44 cases 28 sorted the colors more quickly with the right hand and 8 of the others took the experiment with the left hand from 2 to 4 months after that with the right hand. This gave the left hand a decided advantage. In several cases where children were using forms with the left hand it was clearly apparent that the motor operation could not keep up with the tactual discrimination and numerous mistakes were made on this account. More mistakes were made with the forms than with the colors and this result is believed to be largely due to this fact.

Table VI. shows four left-handed children, 'II.' and 'III.' of whom, however, have been taught to use their right hands. These children were more proficient both in colors and forms

TABLE VI.

COÖRDINATION. LEFT-HANDED CHILDREN.

Subject.	Color.		3 Forms.		4 Forms.	
	R.	L.	R.	L.	R.	L.
I.	261	241	153	155	225	204
II.	198	220	112	110	160	177
III.	200	210	115	125		
IV.	237	270	168	137	208	191

with the right hand, as will be seen. 'I,' however, who has remained left-handed, illustrates our hypothesis completely, the times being in order of decrease; colors right, colors left, forms left, forms right! This is when three forms were used. When the experiment was tried with four forms the results are more ambiguous. This matter is worthy of attention. Several other children reversed their order when four forms were used from that obtained when three forms were used. As the number of forms increases from three to four the motor phase of the coördination problem rises in complexity. There are 24 possible chances here while there are but six in the other case. This is illustrated in the possible ways of arranging the boxes. This side of the problem then is increased in complexity 400 per cent. Now the tactual phase of the problem is not so much affected — only to the extent of 25 per cent. This consideration seems to throw weight in favor of the proposition that in those children who sorted forms more readily with the right hand, the *motor* facility turns the balance — the results might be accounted for even if it were proved that the children had finer sensitivity to touch with the unused hand. Subject 'IV' in the last table is a boy who is not only right-handed, but who insists he has particular difficulty in using his left hand. He is slower in colors with his left hand and swifter on forms with his left hand, both when three forms are used and when four forms are used.

With considerable unanimity the evidence seems to point one way. One point at least is beyond question, *i. e.*, in the

given tests *children discriminate form more readily than color*. In about 100 tests there have been no exceptions to this statement. It is true that in the tests given the form experiment had one advantage in its favor because the child perceives the form immediately upon contact *in the bag* and may at once begin the motor phase of his task. It is not believed that this is the whole story, although this suggests the necessity of a different method of counting time in the experiment. There are strong indications, especially with abnormal children, that form appreciation chronologically precedes color appreciation. Witness, for instance, the cases cited on page 351. Bearing upon this point of the comparative priority and sensitivity of touch and other forms of sensation Dr. Martin W. Barr, chief physician of the Pennsylvania Training School for Feeble-Minded says<sup>1</sup>: "The basis of this scheme of development is the recognition of *touch* as the most sensitive as well as the most reactive of all senses; therefore we utilize it as the master key which shall set free the powers of the head—the hand—the heart." The other point which appears to be true is that the sense of touch is more delicate—at least relatively more delicate as compared with the motor power in the left hand of the right-handed people and the right hand of left-handed people. The interpretation of results of this kind is no simple matter. It is difficult to draw the line between the tactual discrimination and the motor control. It seems impossible to get either in perfect isolation. It would appear that with a right-handed person the right hand would have the same advantage with forms as with color. It may be that the left hand had a slight advantage on the score of practice, for it was the rule to take the right hand first. This statement could certainly be questioned, however, in the light of our knowledge in other psychological fields. This advantage, if any, was greatly counteracted, however, by changing the position of the boxes between tests. Furthermore, it would appear that practice should help in color as well as in form. Another factor, however, worked in the opposite direction to practice. That was fatigue.

<sup>1</sup> 'The How, the Why, and the Wherefore of the Training of Feeble-Minded Children.' *Journal of Psycho-Asthenics*, September, 1899.



In the younger children fatigue would tend to outweigh the value of habit. The novelty of the work soon wore off for them. On the contrary, with the older children the interest was maintained and habit may have helped in the solution of the problem. Further facts will throw light upon the question. The hypothesis is perfectly plausible, theoretically, when there are taken into consideration the comparative passivity of the unused hand and the persistent interpretative mood of the used hand. The theory, moreover, does not require that the unused hand shall be actually more sensitive to touch, but that when the algebraic sum of the sensitive qualities is obtained the tactical has a larger balance in its favor.

At this juncture there is submitted another table, VII., which gives the record of eleven pupils in the Elementary School who were subjects in these experiments in two successive years. The results are shown side by side. Each of these children has been promoted a group during the year and we find a *corresponding increase of ability in the lines of our inquiry*. The table plays very readily into the hands of the general relation already indicated of the power of attention to motor coördination. Table VII., also indicates the value of a method already suggested of studying children which the 'Science of Child-study' has thus far practically ignored. The effort heretofore has been to give a cross-section view of a larger number of children. This method suggests a longitudinal view of a few children as likely to be of more value. If the spider were tested by the cross-section method in motor coördination, some results quite derogatory to the child would be obtained. If the longitudinal method were applied to the spider and the child, the outcome could be easily prognosticated. There is about as much reason in measuring one child by another child as in measuring one child by a spider. The cross-section method is static and has little real meaning. This little meaning will be buried effectually, if the result is dumped in with a mixed multitude of others. The longitudinal method is dynamic; it is a measure of *progress*. The differences, not the resemblances, are the chief value, pedagogically at least. For such a purpose the 'average' is worthless; it is impersonal. When it is



TABLE VII.

Name.	Coordination.		Accuracy.				Rapidity.				Fatigue.	
	1899.		1899.		1900.		1899.		1900.		1899.	
	Colors.	Colors.	Finger.	Arm.	Finger.	Arm.	F.	A.	F.	A.	F.	A.
Clifford	249	189	1° 2' 8"	34' 16"	26' 13"	32' 33"	33	34	35	35	I	97
Edwin	209	208	0° 53'	20' 33"	23' 29"	11' 19"	27	33	29	35	76	66
Fletcher	245	158	1° 46'	18' 49"	33' 49"	2' 51"	23	16	28	35	36	I
William	206	162	26' 31"	0° 12'	41' 18"	13' 8"	43	44	38	46	70	97
Barrett	290	222	4° 8'	23' 55"	1° 7' 38"	17' 6"	31	29	30	34	46	51
Howard	304	205	1° 2' 8"	18' 49"	3° 11' 30"	25' 31"	27	27.5	28	30	66	80
Mary	191	191	44' 20"	0° 12'	0°	14' 16"	31	32.5	30	37	82	50
Donald	252	213	0° 53'	30' 49"	29' 36"	10' 49"	26	30	32	43	60	48
Henry	399	244			35' 20"	15' 24"	30	30	22	27		82
Stephen	460	285			2° 37' 41"	59' 6"	23	15	21	24	72	68
Lander	260	225	1° 18' 33"	37' 41"	1° 18' 33"	17' 6"	12	21	22	29	80	72

NOTE.—'I' = increase, 'E' = even.

considered that each individual is unique in temperament, rapidity and order of his nascent periods, heredity, environment and personal equation, the inadequacy of any 'average' standard is made manifest. The individual's progress should be measured by the possibilities of his own self-realization.

In the discussion thus far we have had under consideration as a rule no smaller unit than the group. Of course it is desirable, if the pedagogical side of the purpose is to be made practicable, to devise a system which will have individual applicability. In this kind of a device two things must be kept in mind. One is this longitudinal method and the other is that the tests, if they are to be of value without repetition, must cover a sufficiently long period of time, or if short in duration, must be of a sufficient degree of complexity. Two of the tests of Mr. Kirkpatrick<sup>1</sup> appear to me to be of questionable value for purposes of individual classification. In one he requires the children to count orally as rapidly as possible for ten seconds; in the other, to make vertical marks as rapidly as possible for ten seconds. The correspondence between the results obtained in this way and those given by the teachers is certainly so remarkable as to throw the burden of proof on any one who might attempt to criticise the tests. But I have not been equally fortunate. My results obtained by the use of the fatigue counter, the readings being taken every ten seconds for a minute, indicate no trustworthy correspondence between the work of the first ten seconds and the child's real mental power, so far as it may be applied to any practical problem of life. It is not only easily conceivable, but results in this study show it to be the case, that the 'C' grade pupils make excellent records for the first ten seconds. Under the novelty of the experiment they pull themselves together and work on borrowed energy as their fatigue curves show. They are among those who have no root in themselves and so endure but for a time. Of 16 pupils marked 'A' by the teachers of the Elementary School ten were graded 'A' by my scale of 'coördination,' 'accuracy' and 'rapidity' results. This is as close a correspondence as any two teachers would be likely to reach in marking the same children, especially when

<sup>1</sup> PSYCHOLOGICAL REVIEW, June, 1900.

it is remembered that the Elementary School pupils are already grouped on the basis of ability alone.<sup>1</sup> But when I came to the pupils marked 'C' by their teachers, of whom there were 11, only two are put 'C' by my system. The 'C' pupils work better at my tasks than they do at the tasks the teachers put them at. This suggests a serious weakness found in every device thus far suggested, so far as the writer knows. The fact of there being in the schools two distinct types of children, *i. e.*, the sensory child and the motor child is ignored. If a motor child does not do the ordinary sensory tasks of the school well is it the fault of the child or the requirement? Leaving for the moment the question of requirements untouched, it seems clear enough either that there must be a different series of tests for the two different classes of children, or there must be found some common denominator in terms of which each class may be evaluated. The standard must not be chosen arbitrarily and it may be arbitrary and still have the custom of centuries behind it. This paper does not claim to have found that standard. If, however, these same 'A' and 'C' pupils are compared on the basis of the fatigue tests, the correspondence sought is marked. These results are computed from the 'rapidity' test, the record of the first and sixth ten second interval being compared. The figures given in Table VIII. show the per cent. which the sixth interval bears to the first interval.

It will be observed first that the 'C's' fatigue more readily than the 'A's,' there being a difference of more than 10 per cent. in both finger and arm in favor of the 'A's.' Both classes fatigue more readily with the finger than with the arm. Of the 10 'C's,' 6 fatigued more readily with the finger. One maintained his rapidity of arm movement and one increased it during the minute. Of the 13 'A's,' 7 fatigued more with the finger, one maintained his rate with the arm and one increased it, while two increased the rate with the finger. The general lesson of the table is clear and unmistakable. While it may be necessary to vary the tests to meet the peculiarities of the motor

<sup>1</sup> Two of the leading teachers of the Elementary School were asked to grade a given list of pupils and there was a considerable degree of variation in the results.

TABLE VIII.  
FATIGUE.

No.	'C' Pupils.		No.	'A' Pupils.	
	Finger.	Arm.		Finger.	Arm.
1	62	55.5	1	88	67
2	66	76	2	73	96
3	48	70	3	93	96
4	94	70	4	67	59
5	82	100	5	66	90
6	76	79	6	85	82
7	89	79	7	86	73
8	53	74	8	142	112
9	89	55.5	9	70	90
10	91	105	10	81	91
Av.	77	76.4	11	91	100
			12	86	87
			13	107	97
			Av.	87.2	88

and sensory child, it would appear that the element of fatigue, if once understood and properly applied, would serve as a practicable common denominator.

*The Emotions.* — A study of the emotional life of abnormal children is of peculiar interest. While the cases in the Physiological School are emotionally erratic as all such children are, they, nevertheless, on the whole possess emotional tendencies. They are much less plastic in their adaptations to their surroundings than are normal children. But for detailed knowledge of the facts of their emotional life we must wait on empirical observation. Experimentation is of limited value.

The abnormal children though lacking intensity in their psychical powers, seem to possess the extent of the normal individual, so that Ireland seems to be justified in saying: 'I do not know of any power which existed in the mind of Shakespeare or Napoleon of which they are totally destitute.' It is also significant that throughout the whole range of the psychic processes here studied the laws of procedure harmonize in an unexpected degree with those already determined for normal adults.

#### SUMMARY.

The following considerations seem to come prominently to light from the work thus far done:

1. There is need of frequent psycho-physical examinations of children. This need applies not only to the neglected classes but to all classes regardless of social condition. Children of the most cultured, the most wealthy and the most alert parents frequently suffer not only physically but mentally because of the unknown physical defects which such an examination would readily bring to light. The amount of mental anguish which children suffer entirely on account of ignorance of parents and teachers is by no means duly appreciated.

2. The prosecution of this kind of work will soon result in the establishment of norms in terms of which a child can readily be scientifically classed for pedagogical purposes.

3. The work of the psychological laboratory is demonstrating that pedagogy will never become a science in truth, until the principle of individualization becomes its watchword.

4. Approximate uniformity of results in psychical reactions is a characteristic of the healthy consciousness. Inability to secure this uniformity is at once a sign of a neurotic condition, which if neglected may become permanent.

5. The study of 'what is in a child's mind' at a given moment is of very questionable scientific value; it is scarcely a psychological study at all. The most of the content of a child's mind at any stated time is determined by his environment. A child of arrested development has a well-developed *automobile consciousness*. He has power of imagery with reference to this machine, visual, and auditory, and motor, which measured in terms of race development alone would indicate a degree of intelligence far advanced. The child's 'religious ideas,' 'ethical ideas,' and so forth are largely the ideas of his elders unconsciously appropriated.

6. The scientific value of tests made on children by those unfamiliar with their habits, vocabularies, environments, etc., is very slight and this value reaches its minimum when the reports are dumped in with others of like character.

7. These tests with both classes of children agree in indicating that touch is a more primitive sense than color. It develops first and maintains its precedence for some years. (How long is as yet undetermined.)

8. Bright colors are generally preferred by these abnormal children.

(The experiments were not extended to the normal children.)

9. The grosser movements of the body develop before the finer ones. There is greater accuracy and rapidity of movement with the shoulder than with the finger and this rule is followed by children up to the highest group (tenth) in the Elementary School.

10. There is a uniform increase of ability at motor coördination as the intelligence rises. This runs through the groups and applies to individual cases.

11. The stress of motor imitation is so strong as frequently to overcome the deteriorating effects of fatigue. See dynamometric readings, page 354.

12. The lower the intelligence the more prominent the element of fatigue appears.

13. The effect of tendencies to rhythm in conscious activity is a very considerable though almost neglected factor in the attempt to teach children.

14. The chapter in children's imagery is yet unwritten.

15. It is quite possible for the simple motor test which discloses the degree of intelligence to be so conducted as to give valuable ethical data as well.

16. The abnormal child is deficient in intensity and not in extent of psychic function.

17. An interesting question is raised as to whether the sense of touch is not relatively more delicate than motor ability in the left hand of right-handed individuals and *vice versa*.



## THE DISTRIBUTION OF ATTENTION. I.

BY DR. J. P. HYLAN.

### I. RECENT STUDIES.<sup>1</sup>

The question as to whether the attention can be distributed or divided has recently found interesting experimental treatment by Jastrow and De Sanctis. Jastrow<sup>2</sup> selected two types of processes to investigate their amount of mutual interference when both were carried on at the same time. The first type consisted of finger movements involving rhythm and counting. The beating of the finger was recorded upon a smoked drum; while the second type, consisting of adding or reading under various conditions, accompanied this. It was found that when the subject chose a convenient rate for the finger movements, the adding or reading did not interfere with them; but when the subject was required to tap at his maximum rate, the mental task always caused an interference. Also simple movements were less affected than those complicated with a rhythm, and reading aloud was more disturbing than reading to one's self. The rate of performing the mental work was measured by taking the time necessary to perform a definite task. Simple regular beats did not increase the time of the mental process, but seemed to hurry it up. But when a rhythm was used in the movements, the time was increased, depending upon the complexity of the movement. The greater the number of beats in a rhythmical group, the greater was the interference. The reading of disconnected words was interfered with more than that of words which made sense. These results show that the more simple and automatic the processes, the less is the mutual disturbance; while the more complicated ones, whether pri-

<sup>1</sup> A concise historical review of this subject may be found in James' 'Psychology' in the chapter on attention. A recent and more extended discussion is by W. Wirth, *Philos. Studien*, XX. (Wundt's 'Festschrift,' II.), pp. 487-669.

<sup>2</sup> Jastrow, Joseph, 'The Interference of Mental Processes,' *Am. Jour. Psych.*, Vol. IV., 1891-92, p. 219.

marily mental or physiological, cause a very pronounced interference.

De Sanctis,<sup>1</sup> whose interest has been especially in the pathological aspects of the subject, has conducted several experiments with regard to the power of fixating and dividing the attention. One method of testing the fixation of attention was to keep the subject busy making movements with the finger, and keep time with the strokes of a metronome, upon which the whole of the attention was turned. These movements were registered upon a smoked drum. While the subject was thus engaged, continually stronger and more numerous distractions were given, and he was told to close an electric circuit whenever these distracting conditions caused an interference. The distribution of the attention was tested in a similar way by employing the subject with two, three or more operations, and directing him to give an equal degree of attention to all at the same time. The movements were registered, and the time employed for the operations was exactly measured.

The objection to this experiment is that one of the processes required no constant attention. Work like Jastrow's indicates how automatic and unconscious the tapping in time with a metronome may be. Movements of this kind are so easily fallen in with that they soon come to require hardly more attention than walking or breathing. Hence the supposition that these simple movements constantly retain a part of the attention is groundless.

Another way of testing the fixation and distribution of the attention, and one which the author regards as clinching the distinction between them, was to use a perimeter, having the subject fixate upon the fixation mark of the apparatus. To test

<sup>1</sup>De Sanctis, Sante, 'Studien über die Aufmerksamkeit,' *Zeitsch. für Psychol.*, Band 17, S. 204.

'L'attenzione e i suoi disturbi. Saggio di psicopatologia clinica,' *Atti della Soc. Rom. di Anthropol.*, IV., 1, 468, 1896. Reviewed by Külpe in *Zeitsch. für Psychol.*, Band 15, S. 144.

'Ricerche psicofisiologiche sull'attenzione dei normali e dei psicopatici,' *Bul. Soc. Lancisiana*, XVII., 2. Also reviewed by Külpe in *Zeitsch. für Psychol.*, Band 19, S. 234.

'Lo studio dell'attenzione conativa,' *Atti della Soc. Rom. di Anthropol.*, IV., 2. Reviewed by Binet, *L'Année Psychologique*, IV., 1897, p. 581.

the fixation of attention, the extent of the field of vision was ascertained when the subject was having read to him an interesting story, or being painfully pricked. To do this, supposing the right eye to be used, the slide on the perimeter would be moved from the right temporal to the nasal side until seen. Successive trials of this kind were used to determine to what extent the visual field was contracted by the distraction. As the meridians are numbered from 0 at the point of fixation towards the temporal side, the greater the reading the larger was the field of vision. In testing the division of the attention, the subject was required to count points, marks or circles exhibited at the point of fixation, and at the same time to attend to the field of vision and note when the object in the lateral field appeared. First, the subject's field was tested under normal conditions with the attention neither distracted nor divided, and the result compared with that when distraction or division was present. An examination of the results of this experiment shows that the extent of the field was somewhat less for distracted than for normal attention; while the average extent for divided attention was less than half that for the normal. The mean variation here also rises to about half the average. According to our author, the contraction of the field is evidence that in divided attention the disturbances are greater than in distraction, and shows conclusively the difference between the concentration and division of attention. Also the division of attention is attended with greater difficulty than its concentration, and in mental disintegration, as with paralytics, the insane, hysterical and aged persons, the power becomes vitiated or lost. The power of distribution thus becomes a prominent feature for psychogenesis, for, developing out of the simpler process of concentration, its accession marks the growth of a greater will power, and the ability to become acquainted with more objects in a shorter time.

I have gone into some of the details of this work because it is on an important subject, and has attracted a good deal of attention, especially in Europe. There is a serious criticism to be made regarding it. It is that the experiments are altogether too crude to deal with the subtle conditions of the problem.

The evident objection to the perimeter experiment is that it is impossible to tie the attention down when there are no distractions, while the difficulty is more obvious with them. It is continually flitting back and forth with reaches of varying breadth and minuteness, and with changing frequency and rapidity. What the experiment with distraction tested was the time and frequency of the fluctuations of the attention from the temporal field with the distracting stimulus as compared with these when the distraction was absent. With an effort to keep in mind the temporal field, the attention was intermittently diverted from it by the distraction, and the object in the temporal field allowed to move in towards the nasal side farther without being noticed than was the case when the distraction was not present. When an effort was made to divide the attention between the lateral field and the fixation point, the fluctuation was naturally more frequent than before, and at the expense of the lateral field, which now retained it for shorter periods and at longer intervals, thus allowing the object to move still farther without being noticed. Another condition which contributed to this result was the fact that counting involved a much more continuous mental effort than the listening to a story or feeling one's self pinched, the agencies used in the distraction experiment. Hence, on this account also, the lateral field was, in the case of division, more neglected. It is but to be expected, therefore, that under these conditions the object in the temporal field could often be moved very far towards the nasal side without being noticed, or that it sometimes reached, as the figures show, clear to the *o* meridian.

This interpretation is suggested by an experience of several years which have been devoted to experimental study of problems of attention. It is supported by the extraordinary variation shown in the division experiment. If the attention were really divided, why should the object in the temporal field be seen sometimes as quickly as under normal conditions, while at other times it was not seen until it had reached the median plane? Evidently because sometimes the attention was on the temporal field, while at other times it was not.

What becomes, then, of De Sanctis' theory and its significance for psychogenesis? Whatever may be its foundation

upon other data, it certainly is not justified by these experiments. Any one can readily discover the great effort necessary to balance one's attention so that its fluctuations shall be rapid and regular between two or more objects. The difficulty of this feat is a sufficient reason why the demented are unable to perform it.

Other authors who should be mentioned are Münsterberg, Binet, Loeb and Krohn. If we discover the time (by the reaction method) needed for naming a member of a single class of objects, as giving the name of an American novelist, and also the time needed for making a comparison, as saying which is liked better, Irving or Cooper, we shall have the time for two different mental acts, each determined separately. The subject may now be required to do both these acts in one, as by telling which of the American novelists he likes best, and the time taken for the combined process. Münsterberg combined acts in this way, and found that while it took 103  $\sigma$  to name a particular member of a class, and 922  $\sigma$  to make a comparison, it required but 1,049  $\sigma$  to do both together, a saving of 46  $\sigma$  over the sum of the times when done separately.<sup>1</sup>

Binet required his subjects to press regularly a closed rubber tube which caused a tracing on a smoked drum. While this was being done the subject was required to execute some mental work, like reciting by heart, or performing a mathematical calculation. It was found that these operations greatly disturbed the regularity of the compressions. When different tasks were given to the two hands to do at the same time, as that of making two different outline drawings, one drawing would be deformed by similarities from the other. A voluntary process, however, could well be accompanied by one that was purely automatic.<sup>2</sup> Loeb describes experiments quite in accord with these results, and also with those of Jastrow. The rhythm in turning a wheel and that of repeating at the same time verses by heart were found to coincide, or one to be a multiple of the other, and without mutual interference; while raising the pressure on a dynamometer to its maximum was

<sup>1</sup> 'Beiträge,' I., S., 64-188.

<sup>2</sup> 'La Concurrency des états psychologiques,' *Revue Philosophique*, Vol. XXIX. (1890), p. 138.



found to interfere with the performance of mental number work. Thus two simultaneous, maximal, aperiodic processes of innervation which require effort were found to disturb each other.<sup>1</sup> Krohn gave ten simultaneous touch sensations to different parts of the body. Tambours carrying corks as instruments of touch were used, which immediately withdrew when the stimuli had been given. It was found that if the points of contact were somewhat scattered, and the subject attended closely, six out of seven simultaneous touches could be clearly grasped and correctly localized. It should be said, however, that after-images of touch were very persistent and were used to a considerable extent in locating the sensations.<sup>2</sup>

These experiments suggest the need of more critical and exhaustive methods of studying the problem in order that the purely mental factors involved may be given more accurate measurement. Other experimenters will be discussed as occasion suggests. There is also a class of pathological cases which has been interpreted as illustrating a doubling of the mental process. Lack of space prevents their discussion here, although I believe they differ from the normal in degree rather than in kind. At the present time there is an almost universal tendency to believe that the power of division is possessed by the normal mind. We have now to consider some new evidence bearing upon this question.

## 2. COUNTING SIMULTANEOUS SERIES OF SIMILAR IMPRESSIONS.

From the foregoing discussion it is evident that two conditions must be fulfilled in devising experiments to test the division of attention. First, no dependence should be placed upon the subject's ability voluntarily to divide his attention; and second, if the mental work employed is continuous, the purely mental processes involved, the simultaneity of which is to be tested, must be accurately measured. In conformity with these principles, the following experiments were planned and executed:

<sup>1</sup> 'Comparative Physiology of the Brain and Comparative Psychology,' p. 289, *et seq.*

<sup>2</sup> Krohn, W. O., 'An Experimental Study of Simultaneous Stimulations of the Sense of Touch,' *Jour. of Nerv. and Mental Diseases*, 1893.



A revolving kymograph drum held horizontally had placed upon it a paper having two series of short horizontal lines extending partly around the drum. A screen, placed closely in front of this, had in it two small openings a centimeter apart, and so placed that when the drum revolved, the lines upon the same could be seen one at a time through the openings; each series of lines being adjusted to its own opening. A fixation mark was placed half way between the openings, and the subjects arranged at a convenient distance for counting the lines. With the eyes of the subject fixed upon the fixation mark and the attention upon one opening, the other being closed, the drum was started at too rapid a rate for correct counting of the lines. A signal was given before the first line appeared, and the rate gradually adjusted for the successive repetitions of the series, so that the maximum rate of counting at which the subject could count correctly and feel a fair amount of certainty in the correctness of his work was ascertained. The exact number of lines was known only to the experimenter. This process was repeated five times for each subject. The time taken for the passage of the whole series past the screen opening was taken with a stop-watch, and this divided by the number of lines in the series, giving the time for each line. The average and mean variation of this for the different trials were then reckoned.

In a similar way, with the eyes fixed as before, and both screen openings in use, the time was taken for counting the lines which appeared in both openings, both series being exposed simultaneously. In this part of the experiment the subjects were directed to divide their attention, if this were possible, between the two openings. Four subjects took part in this experiment, two men and two women, all young, vigorous, and having had some training in experimental work. The mental part of the process was never so rapid as to be delayed by the natural motor accompaniment in the vocal organs, and the number counted was always approximately the same, aggregating from thirty to forty, either when one or more than one series was used, in order to avoid inequalities of fatigue. When more than one series of lines was used this number was divided about equally between the different series. As it was found

impossible to keep a separate count for each series when more than one was used at once, a single count was kept for all.

In addition to this double series, a triple and also a quadruple one were used, having three and four openings respectively, for the purpose of ascertaining the effect of an effort for the greater distribution of the attention, and comparing this with the single series. Similar methods and precautions were used here as those already mentioned, although only three instead of four subjects took part.

At first, when more than one opening was used, there was a strong tendency for the eyes to turn directly to the openings in response to the lines as they appeared. After practice had served to correct this, there still remained a responsive shifting of the attention which could not be prevented. A rhythm appeared in the counting which materially aided its rapidity. The nature of this rhythm depended largely upon the order in which the lines appeared, and was hence facilitated by an acquaintance with the series. A feeling of certainty in the correctness of the counting arose in connection with this rhythm, and depended upon the coincidence of the rhythmic beats with the appearance of the lines.

Since the time required for the passage of the lines as a whole was taken for each series, and the average time for the counting of each line in each series was reckoned from this, we have a basis of comparison for the different series. When the time of the different subjects was averaged, the time required to count a single line was found to be  $437\sigma$  for the single series,  $307\sigma$  for the double series,  $278\sigma$  for the triple series, and  $1021\sigma$  for the quadruple series. The mean variations were  $9\sigma$ ,  $14\sigma$ ,  $11\sigma$  and  $8\sigma$  respectively. No marked tendency to improve the rate by practice showed. In the single and double series, the order in which the lines were shown presented no difficulty in being formed into a rhythm that could be remembered. This was also true of the triple series if the order were not too complicated. A triple series, made too irregular to allow being remembered, was used, which required  $483\sigma$ , or nearly twice the time required for the simpler triple series already reported. The mean variation for this was but  $6\sigma$ . The quadruple series was also too complicated to be remembered.

In order to control any details of this result which might come from the peculiarities of the visual sense, the problem was also approached by means of auditory sensations. A toothed wheel was made to revolve by an electric motor, the speed being reduced by an extensive gearing. This, with the additional assistance of a resistance bridge, made it possible to vary the rate of rotation as desired. A light steel spring was clamped in such a position that as each tooth of the wheel passed, a musical tone, having a distinct pitch, was sounded. One side of the wheel was partly covered by a non-conductor, and a metallic arm made to press against the side of this as it revolved. The arm and wheel were put into an electric circuit with a sounder, which was thus made to give a signal for the subject to start and to stop counting the clicks. The breaking of the current, causing the signal for starting, was made by the passing of the non-conducting section of the wheel by the arm; and the making of the current, causing the signal for stopping, was caused by the re-instated contact through the wheel.

The method of procedure was the same as that for the last experiment. The motor was first started briskly and then slowed down until the subject could give correctly the number of clicks sounded. This was repeated five times and very great care taken to make each determination accurate. A second, third and fourth series was formed by having two, three and four springs respectively used at once, each giving a distinctly different pitch, none sounding at the same time, but in succession. The tendency to form a rhythm was especially pronounced in this experiment. Since a distinct rhythm tended to make the clicks fall into groups, and these groups rather than the individual clicks to become the basis of counting, great care was needed in the arrangement of the apparatus to avoid an objective rhythm. In spite of this a subjective rhythm continued to be more or less in evidence, though not of an extreme form. Two subjects took part in this experiment. As practice appeared as a significant factor in the problem, its effect was not eliminated by means of preliminary series of experiments, but distributed between the different series by the method of rotation, *i. e.*, each subject taking one determination in series

one, two, three and four consecutively, and then beginning with one again, and thus continuing.

According to the most probable theory of the sensations of pitch, each minor center composing the auditory area in the brain gives a slightly different pitch-sensation in response to the variations in the vibration rate of the stimulus. Hence we would have, *e. g.*, in the quadruple series of our experiment, four brain centers acting in response to the four rates of vibration set up, and producing or correlated with the consciousness of the four pitches. If the attention can be divided, it must act as a multiple consciousness based upon the simultaneous activity of these different centers: and the possible sum of counted sensations in this multiple consciousness should be four times as great in the quadruple series as in the single series for the same lengths of time. We can see how this proportion might readily correspond to the amounts of brain disturbance under these respective conditions. The essential question is as to whether this four-fold disturbance can articulate itself into a four-fold correlated consciousness. Evidently the same argument holds for visual as for auditory sensations.

What came from this experiment with clicks was a gradual increase in the time necessary for counting each click in passing from the single to the quadruple series, the opposite of what would be expected if the attention had been divided. As in the last experiment, the time necessary for counting the sensations in each series was taken, this divided by the number in each, and the time thus found for counting each sensation averaged between the different subjects. We thus get for the single series,  $165\sigma$ ; double series,  $180\sigma$ ; triple series,  $182\sigma$ ; quadruple series,  $200\sigma$ . The mean variations were  $16\sigma$ ,  $30\sigma$ ,  $23\sigma$ ,  $16\sigma$ , respectively. The effect of practice was general, although most pronounced in the double and quadruple series.

Since in the visual experiment less time was required for the double than for the single series, and less for the triple series, when this could be remembered, than for the double, there is an obvious disagreement between the results of these corresponding series in the two experiments.

In order to test this point as fully as possible, a single and double series of touch sensations were also tried. The toothed wheel used in the last experiment was also used here, but without the steel springs used to give the auditory stimulations. In their place were substituted cardboard rests with small openings in them, and so arranged relative to the rim of the wheel that when the fingers were placed against the openings of the rests, each tooth of the wheel in passing caused a touch sensation. The finger nails of the forefingers were used for this instead of the fleshy parts of the finger tips, since it was found that the latter fatigued, and confused the sensations much more readily. In general, the methods formerly used were also employed here. There were three subjects in the experiment.

With the single series of touch sensations no rhythm was present, only a simple series of unrhythmic touches being felt. For the double series a subjective rhythm appeared for all the subjects, and this seemed to assist somewhat in the counting. The average time for counting a single sensation was  $185\sigma$  for the single series, and  $189\sigma$  for the double, numbers which confirm the results of the auditory experiment. The mean variations were  $6\sigma$  and  $18\sigma$ , respectively.

### 3. COUNTING SIMULTANEOUS SERIES OF DISPARATE IMPRESSIONS.

In his experiments with simultaneous mental processes Paulhan found that dissimilar operations conflicted less than similar ones.<sup>1</sup> In order to give the method thus far described as exhaustive a trial as possible, a disparate series was arranged, composed equally of visual, auditory and touch sensations. The toothed wheel before mentioned was used. One steel spring and one finger-rest were arranged for giving a single series of auditory and touch sensations respectively, while a single series of visual impressions was furnished by an arrangement of lines on the side of the wheel, before which was placed a screen with a small opening. The electric sounder gave a signal for starting and stopping. As in the other cases the subjects, of whom there were three, were directed to distribute the attention

<sup>1</sup> *Revue Scientifique*, Vol. 39, p. 686 (May 28, 1887).



equally, so far as this was possible, upon all the three kinds of stimuli. Upon first trying this experiment, the subjects experienced extreme confusion, unlike that caused by the other experiments, and so great as to make it impossible to count at all, except after some practice. After this had become possible, the rate was much slower than for any other series, although it rapidly improved. The average rate for the three subjects per stimulation was  $333\sigma$ , while the mean variation mounted to  $77\sigma$ , an amount largely accounted for by the rapid improvement in the rate of counting. The amount of this increase of rate is shown in Table I., where the time in seconds for counting the whole series for each successive time is given for each subject. It will be noted that a point is soon reached at which improvement

TABLE I.

Subject.	<i>H.</i>	<i>F.</i>	<i>D.</i>
1st time.	14.25	16.25	8.00
2d "	14.00	10.50	8.25
3d "	7.50	7.00	8.25
4th "	6.50	7.00	8.00
5th "	6.50	7.25	8.00

stops. With subject *D* this point had apparently been reached in the preliminary practice before records were begun to be taken.

This comparatively slow rate, and especially the confusion, indicate the relative difficulty of combining dissimilar processes, and hence is opposed to Paulhan's results. There is, however, this essential difference between his method and my own. In the experiments described above there was one motor expression for both processes, while with him, as in the case of writing one verse and repeating another, the processes had unlike motor expressions. In attempting, he says, to write one verse of poetry and recite another, sometimes strange mixtures of the two would appear in the writing, but this was not often, at least when the same elements did not enter into the two. He continues: "The words which form a line and the lines which compose a piece each hold well together; in general, always in reciting, I recall one or two features of the lines which I wish



to write; after that I think no more, the writing follows mechanically." He describes multiplying figures with one hand and reciting poetry at the same time. He multiplied 421,312,212 by 2, which took six seconds. The recitation of four lines of poetry also took six seconds, but both were done together in six seconds. In trying to make two multiplications at the same time, one with the right and one with the left hand, time was lost. The two operations done at once took thirty-eight seconds; while when done separately, one took fifteen seconds and the other eight.

His experiments seem to have been many and carefully performed. He concludes that 'the most favorable conditions for doubling the mental process appears to be the simultaneous application of the mind to two easy operations and of a different kind.'

When the same elements entered into the two processes the motor expressions evidently became mixed, while when the processes were widely different this was not the case. Thus with divergent motor paths Paulhan found dissimilar processes to be more successful, while with convergent paths we have found similar processes to be more successful. From this we may readily infer that converging paths are more suitable for similar processes, and divergent for dissimilar ones. We have now the question as to whether distribution of attention really took place in these most favorable cases. The following table gives a more detailed view of the results of these experiments. The numbers denote thousandths of seconds required for counting a single sensation. Each number is an average from five determinations.

#### 4. WAS DISTRIBUTION PRESENT?

The table shows a striking uniformity between the different subjects employed. Thus all show a decrease of time in the double and first triple series in the visual experiment; all show gradual increase as the complication increases in the auditory experiment; the time remains nearly constant or the same for the two series in the touch experiment; and all show a marked increase in the combination experiment. The auditory experi-

TABLE II.

Subject.	Hy.		Ka.		Ko.		B.		F.		D.		All.	
	Av.	M. V.	Av.	M. V.	Av.	M. V.	Av.	M. V.	Av.	M. V.	Av.	M. V.	Av.	M. V.
One opening.	301	9	385	10	693	3	361	16					437	9
Two openings.	242	9	309	10	376	31	299	4					307	14
Three openings.	292	23	261	16			280	4					278	11
Three openings with irregular arrangement.	435	4	458	5			555	9					483	6
Four openings.	901	18	899	1			1263	5					1021	8
One click series.	171	13							150	18			165	16
Two click series.	197	34							162	26			180	30
Three click series.	200	34							164	12			182	23
Four click series.	220	24							179	7			200	16
One touch series.	195	5							180	7	181	5	185	6
Two touch series.	195	5							180	7	192	24	189	18
Combination.	388	129							328	97	279	5	333	77

ment shows the least time and a generally large mean variation. Here the rhythm was most difficult to suppress, a fact which partly accounts for both of these features; for when the rhythm controlled the counting, the tendency to combine single clicks into groups caused a marked increase in the rate, while the successful attempts to suppress the rhythm caused a decrease of rate, and hence made a pronounced variation. It was very difficult to tell when a rhythmic grouping took place. Obvious rhythms were avoided, but it was so difficult to draw a line between the rhythmic and non-rhythmic that it was often difficult in this experiment to know when the counting was properly performed.

It is the difference between the rhythmic and the non-rhythmic counting which probably explains the decreased time of the double and first triple series of the visual experiment and the resulting conflict between these and the corresponding series of the other experiments. With the two and three screen openings of the visual experiment the rhythmic possibilities were greatly increased over the single opening, while the complexity of the series was not necessarily so great as to preclude remembering it, as was the case with the quadruple series. Hence the introduction of a rhythm, with its natural accompaniment of an increased mean variation, caused the greatly increased average rate. In sharp contrast with this double and first triple series is the irregular triple series, which could not be remembered, and where the time was greatly lengthened and the mean variation correspondingly decreased. It would seem to be this, rather than an economic distribution of the attention, which accounts for these decreased numbers and increased variation in the double series, the only ones upon which evidence of a simultaneous distribution could be placed. Indeed, in all the series in which we might have supposed distribution to be possible, the subjects experienced a rapid fluctuation, or oscillation of attention, a fact that of itself argues strongly against distribution.

But did not distribution take place in Paulhan's experiment with dissimilar processes and with motor paths diverging to different organs of expression? It should be noted that Paulhan

speaks of simultaneous psychic or conscious acts rather than of simultaneous acts of attention. If we restrict all psychic acts to acts of consciousness, and all acts of consciousness again to acts of attention with their varied aspects as voluntary and involuntary, active and passive, then Paulhan's experiments test the division of attention. I have assumed this much and have regarded attention not as something added to consciousness, but as the character which it from time to time takes on.<sup>1</sup> Our author, however, seems to restrict attention to its more active and voluntary aspects, and to regard it as a phase of mental activity added to consciousness or much narrower than it. Thus, while he notes that the attention often oscillates in his experiments, he does not regard this as a reason for denying the simultaneity of two acts of consciousness. The fact, also, that at least one of the simultaneous processes must be learned by heart as a preparation for the experiment, raises the question as to whether consciousness is necessarily involved in both at the same time, and also places great hindrance in the way of any economic value which might rise from the practice.

One explanation may be ventured as to the cause of the decreased rate of counting the disparate impressions in the combination experiment, and of the same effect in passing from the simple to the complicated series with similar impressions. We know that there is an inertia which affects the functioning of the nervous system. This is illustrated by the time it takes for sensations to become fully felt, and for the transmission of pain. Cattell has estimated that it takes from  $47\sigma$  to  $58\sigma$  simply to become conscious of a small object, as a letter upon a white surface.<sup>2</sup> In reaction experiments a preliminary signal needs to precede the signal for reaction by about  $1\frac{1}{2}$  seconds, in order to give the attention time to reach the right intensity for the quickest response. When no preliminary signal is given, sensorial reaction is lengthened  $26\sigma$ .<sup>3</sup> It thus takes an appreciable length of time for a center to respond fully to a stimulation.

<sup>1</sup>I have briefly developed this idea in 'The Fluctuation of Attention.' *PSYCH. REV., Mon. Sup.*, No. 6, p. 62.

<sup>2</sup>*Mind*, Vol. XI. (1886), p. 383.

<sup>3</sup>Wundt, 'Physiologische Psychologie,' II., S. 348.

When, in the above experiments, the intermittent stimulations were most frequent, as in the cases of the double series of auditory impressions where the fluctuation was between two tones only, the centers were kept in nearly as constant a state of excitation as when the series was single; but when, instead of two, there were three and four centers to share these, the frequency for each center must have correspondingly diminished, and hence the impressions occurred when these centers were in a less stimulated, and hence less responsive, condition. Therefore it took a longer time for them to act, and the rate of counting was correspondingly decreased. The presence of rhythm probably explains the absence of this effect in the visual series, where rhythm was not so carefully guarded against, and where the rate of counting increased with the complication of the experiment.

This perhaps explains the gradual increase of time needed for counting the series as they became more complicated, but another cause also made the combination series slow and distracting. This was the difficulty of combining unlike sensations. If one tries to attend, *e. g.*, to a visual and an auditory stimulus at the same time, he will notice a greater difficulty to attend the effort than when the sensations are similar. The mind apparently oscillates between the two rather than distributes itself between them in the case of both similar and disparate sensations, but with the disparate sensations the amplitude of the oscillation seems greater and requires a longer time. Practice, however, rapidly increased the rate of the combination series, apparently by decreasing the time of this transition, a result common to all associative processes.

##### 5. REACTIONS WITH CONCENTRATION AND DISTRIBUTION.

One experiment or one kind of experiments is not sufficient, however conclusive it may be in itself, to test so general a proposition as that of the distribution of attention, because of the great variety of conditions involved, and of processes which may be combined. An experiment was accordingly devised to test by numerical results whether or not the attention really fluctuates from one to another of two or more objects upon which an effort is made to distribute it.



A cardboard screen was fixed in a vertical position upon a table, with six small openings arranged in a horizontal line three cm. apart. In the middle was a fixation point. Behind these was an electric sounder, muffled to be noiseless, and with a white bob attached. This was so arranged that the bob appeared behind one of the screen openings when the current was broken. The breaking of the current started a chronometer (Verdin), from which thousandths of seconds could be estimated. The subject was placed three feet from the screen with his eyes upon the fixation mark. Upon seeing the object through the screen opening, he reacted and closed the current.

With the subject's eyes always directed towards the fixation mark, a series was taken in which the subject knew in which opening the object would appear and had his attention on it; and also a series in which the opening in which the object would appear was not known, and when the attention was distributed, so far as possible, upon all the openings at once. A preliminary 'ready' was given before each signal, and the reactions were sensorial rather than motor. In order to avoid any influence from the fact that some openings were seen more indirectly than others, the signals in both series were divided equally between the different openings. Three subjects took part in the experiment, and one hundred reaction times were taken from each in each series.

An examination of the conditions shows that the only difference in these two series was that in one the attention was concentrated, while in the other it was distributed over the six openings, so far as this could be accomplished. We may, therefore, assume that the differences in the results of the two series would come from the effort at distribution. If the introspective record and the interpretation of the numerical results in the counting experiments were correct, we should expect an increased reaction time in the unknown as compared with the known series. This would be due to the unavoidable fluctuation rather than distribution of the attention, in the unknown series, between all of the six openings. The reasons why this fluctuation should cause a lengthened reaction time would evidently be similar to those for the increased time of counting as



the complication increased in the counting experiments. An examination of Table III. shows this expectation to be realized. The average reaction time for the known series was 161 $\sigma$ , while for the unknown it was 178 $\sigma$ , or 17 $\sigma$  greater.

TABLE III.

Subject.	<i>Hy.</i>		<i>D.</i>		<i>F.</i>		<i>All.</i>	
	Av.	M. V.	Av.	M. V.	Av.	M. V.	Av.	M. V.
Opening known.	160	20	147	17	177	17	161	18
Opening unknown.	164	26	157	23	214	17	178	22

The objection, however, may be advanced that the distribution of attention does not necessitate the giving of the same amount to each of the several objects between which it is distributed as would be given to a single object when that received it all. A decreased intensity, instead of a fluctuation, would thus account for the increased reaction time in the unknown series. Hamilton, says attention is subordinated to a certain law of intelligence. "This law is, that the greater the number of objects to which our consciousness is simultaneously extended, the smaller is the intensity with which it is able to consider each."<sup>1</sup>

There is a way of testing the validity of this objection. If, when the effort is made to distribute the attention among several objects, it really fluctuates from one to the other, naturally, it would sometimes, in the unknown series, be upon the right opening when the signal appeared, but more often upon the wrong one. In the former case the reactions would be short, while in the latter case they would be unusually long. In other words, the mean variation would be somewhat greater in the unknown than in the known series. The table shows this to be the case; there being a greater variation by 4 $\sigma$  for the unknown series. Here the exact amount of increase in the mean variation is evidently a matter of chance, since the attention could be expected to be upon the right opening but one time in six, and might be so many less than that as to make no marked increase in the variation. In this case, however, when the right opening would be chanced upon but seldom, the average reaction time would be distinctly increased. A glance at the table

<sup>1</sup> 'Lectures on Metaphysics and Logic,' Vol. I., p. 164.

will make it apparent that this was the case with subject *F*, whose mean variation is the same for both series, but whose average reaction time is increased in the unknown as compared with the known series  $27\sigma$  more than for any other subject.

I believe it would be difficult to devise a more convincing test than this experiment affords. Yet, as an effect of practice, there is a tendency for the results to change. In the combination series of the counting experiments, it will be remembered that practice had a marked influence in increasing the rate of counting, an effect presumably resulting from the use of the association paths relating the centers involved. A tendency appeared in the reaction experiment just described, which calls for a similar explanation. At first, when trying to distribute the attention in the distribution series, the fluctuation between the different openings was prominent, but as the experiment continued it became less so, and finally became almost unnoticed. Even the giving of the signal in an unexpected opening in the known series secured a reaction not appreciably lengthened. This raises the question, to which we shall return, as to whether practice may not be the means of making distribution possible, or if it serves to combine into a psychic unity things at first separate and unrelated.<sup>1</sup>

Wundt describes an experiment similar to the above, in which loud and weak sounds were irregularly interchanged as the signal for reaction. As the subject did not know which signal would be given, he was unable to prepare exclusively for the right one. There was an increase of  $122\sigma$  in the reaction time over the average of  $121.5\sigma$  when the right signal was

<sup>1</sup> The present study was continued for a period of four years, although during one of these years experimental work was suspended as it was impossible to take up the work at laboratories visited in Germany. The experiments thus far described were performed at the University of Illinois in 1898. While the present author directed and took part in them, they were personally conducted by Mr. J. M. Fisher, to whose ingenuity and enthusiasm much credit is due. The experiments which follow were performed later at the Harvard Psychological Laboratory. I am indebted for many suggestions to Professor Münsterberg, through whose courtesy the resources of the laboratory were placed at my disposal, causing the successful continuation of the work, and for the able and patient assistance received from the many subjects who took part in the work.

known, while the mean variation showed an increase of  $46\sigma$ .<sup>1</sup> A valuable experiment similar to this would be to have the subject's attention directed to several disparate sensations instead of all being of the same kind. The same author mentions one of this kind in which sensations of light, sound, and touch were employed. No numerical results are given, although a very noticeable lengthening of the reaction time is mentioned, and also a continual fluctuating of the attention between the different senses.<sup>2</sup> He does not mention these experiments in reference to the question before us, although their results are obviously in accord with those here described.

#### 6. TACHISTOSCOPIC EXPERIMENTS.

On the other hand, Wundt has contributed the most formidable experiment we have in support of the theory of division. Its general features resemble Hamilton's well-known experiment, in which the attention was turned to a handful of marbles thrown upon the floor and the attempt made to observe them all, although it has fewer technical objections. The apparatus used was named the *Tachistoskop* by Volkmann. Cattell made an improved pattern,<sup>3</sup> while a still more improved form is described by Zeitler.<sup>4</sup> The essential features of the apparatus consist of a shutter having a rectangular opening in the middle, and sliding up and down in the grooves of two parallel uprights. There is a card holder arranged behind the shutter, so that when it is raised the card, upon which are placed letters or figures, is hidden by the lower part of the shutter. When it is in this position, the subject's eyes are directed to a fixation mark on the shutter, and over the center of the card. After a given signal the shutter falls, and in doing so exposes the card through the middle, and then immediately covers it over with its upper part. The apparatus thus serves to expose the letters on the card from  $76.2\sigma$  to  $93.7\sigma$ , those at the bottom being exposed the shortest time on account of the acceleration of the falling

<sup>1</sup> From figures on page 351 of the 'Physiologische Psychologie,' Bd. II., 4th edition.

<sup>2</sup> *Ibid.*, S. 352.

<sup>3</sup> *Phil. Stud.*, III. (1886), S. 94.

<sup>4</sup> *Phil. Stud.*, XVI. (1900), S. 380.

shutter. The theory of the experiment is that the number of letters which the subject can give from the exposure indicates the number of ideas that can be apperceived or attended to simultaneously.<sup>1</sup>

Consciousness is believed by Wundt to have wider limits than apperception. To test this limit, a specially arranged metronome gives a series of single beats, and it is found how many of these can be correctly judged as equal to another series which immediately follows it. As these series increase in length, a point is reached at which the subject can no longer judge correctly of their relative lengths. Sixteen single, or eight double strokes form this limit. Hence, sixteen is the number of separate impressions that can be held in consciousness at once.<sup>2</sup>

The number of separate objects that can be apperceived at once with the tachistoscope is given as varying from four to five. Six is usually considered the extreme limit, although this may be greatly increased if letters are used and they are arranged in intelligible syllables. Unfortunately we are without an explanation of the limit of this multiple activity of the attention, so that little satisfaction could be given one who should ask why ten or a dozen objects should not be simultaneously attended to as well as four or five.

We have the question as to whether the attention is really divided under the conditions of Wundt's experiment, or if the results from it are susceptible of a different interpretation. It came to be my purpose, therefore, to produce variations of the experiment in order to ascertain the real nature of the processes involved.

When one performs the experiment described by Wundt, a slight hesitation is often noticeable before the number of letters or simple objects seen can be named. This suggests that the objects are not clearly perceived during the time of exposure as Wundt claims to be the case. To test this, a tachistoscope was made embodying the essential features of the different forms of Wundt's apparatus, but avoiding the acceleration of the shutter

<sup>1</sup> 'Physiologische Psychologie,' B. II., 286 ff.

<sup>2</sup> *Ibid.*, S. 286 ff.

which would cause some letters to be exposed longer than others. Its front consisted of a black hard-wood screen, 20 by 40 cm. and 0.6 cm. in thickness, so supported upon a base that when placed upon a table it was perpendicular to the sagittal axis of the subject seated before it. In its center was an opening 6.3 cm. square, with bevelled edges to avoid shadows. Behind this an oblong shutter of similar material, with a bevelled square opening in the middle matching the first, was made to slide horizontally instead of vertically, as with Wundt's apparatus. A heavy rubber band attached between the forward end of the shutter and a small windlass at the outer edge of the screen furnished a means of propelling the slide past the screen opening, and of varying its rate in proportion to the tension of the band. A card-holder was arranged behind the shutter so that objects on the card were exposed as the shutter passed. A lever released the shutter, and a spring and rubber cushion stopped it. The rate of movement was ascertained for all parts of the passage by means of a smoked paper attached to the shutter and which passed by a vibrating tuning-fork. This showed that for about the first and last inch of the movement there was an acceleration and retardation respectively; but that for the middle part of the movement, which was used for the exposure, the rate was constant. Fig. 1 represents the back of this apparatus.<sup>1</sup> Wilson's black gummed letters, 7 mm. in height, were used upon white cards as objects to be exposed.

#### 7. LETTERS EXPOSED IN SUCCESSION.

If the letters are exposed in succession, one at a time, but appear to be exposed simultaneously, evidently they are not perceived until all have been exposed, *i. e.*, the act of perception would not take place during the exposure, but after. To determine this, the opening in the shutter was, by means of the insertion of a blackened metal plate, closed to a narrow slit 7 mm. wide and extending vertically the width of the opening. Twenty cards were used and six letters were arranged irregularly upon each card, but so that as the opening in the shutter passed in front, only one letter was shown at a time. Thus while the let-

<sup>1</sup> The figure will appear in Part II. of this article.



ters were placed in all parts of the square surface of the card exposed, they were exposed in a definite order of succession. The time of the whole exposure was  $20\sigma$ , or approximately  $3.6\sigma$  for each letter. It should be noted that, owing to the method of exposure, first the first part, then the whole, and finally the last part of each letter was uncovered. The time when the letters as a whole appeared was extremely short, as the width of the opening was but a little greater than that of the letters.

A practice series of considerable duration preceded the regular experiment, although the effect of practice was not very marked. The method of procedure was as follows: The subject was placed at a constant distance from the apparatus, and kept in position by a head-rest. This distance was 1 m. for all but one subject (*R*), who required to be but 60 cm. in order to get a distinct impression. The eyes were fixed on a fixation mark placed on the shutter and in front of the center of the area to be exposed, and the attention was distributed as far as possible over this area. The experimenter gave a double signal, one two, and one one second before releasing the shutter. The subject gave the letters which he saw in the order in which he saw them, and also their locations in the field in order to keep account of those wrongly seen. In order to guard against fatigue, a short recess was given after each ten exposures, and only thirty exposures were given in an hour, as more than that was found to be fatiguing. Each of the twenty cards used was exposed five times during the series, thus making a hundred exposures for each subject. A record was kept of the letters seen correctly, their order, those seen wrongly, and those misplaced. When this series had been completed, another was taken, in all points similar, except that the shutter was made to move from left to right during the exposure instead of from right to left as in the first series. This served to reverse the order in which the letters were exposed. No succession in the exposure of the letters was perceptible by the subjects. It might be, however, that the order of exposure unconsciously influenced the order in which the letters were perceived, and this would show that perception took place, partly



at least, during the exposure. It was to test this that the changed order of exposure was tried.

Table IV. gives the numerical results of the two series, showing the number of letters seen correctly, those seen wrongly, and those misplaced in the hundred exposures for each subject in each series. Of those misplaced in the consecutive exposure,

TABLE IV.

Subject.	A.	H.	R.	S.	Y.	Av.
Consecutive Exposure.						
Number seen correctly.	212	222	158	160	224	195
Number seen wrongly.	40	30	58	50	58	47
Number misplaced.	17	16	5	18	23	16
Reversed consecutive.						
Number seen correctly.	208	225	167	151	229	196
Number seen wrongly.	32	22	64	53	52	45
Number misplaced.	6	13	7	10	9	9

75 per cent. were otherwise correctly seen, while in the reversed consecutive this number amounted to 69 per cent. The average number wrongly seen is about equal in both series, and also the number seen correctly. These features of close correspondence show that the conditions in both series were practically equally favorable for seeing the letters. If we divide these numbers by 100, we shall get the figures for a single average exposure. This would make the average number seen correctly slightly less than two, the smallness of which we shall see later is readily accounted for by the short exposure. Ten subjects took part in the first series, but since only five were employed in the second, and the results were uniform, only five are given in the table. All of the subjects were men, the most of whom had had considerable laboratory training.

It is, however, in the order in which the letters appeared and the introspective records that we find the most significant features of the experiment. Some features of the mental process would be more distinct with one subject than with another, and the experience would vary somewhat for different exposures, but a general harmony prevailed throughout. Uniformly at the time of exposure the card would seem to flash out without one side appearing before the other. Hence perception evidently

did not take place until after the exposure was over. The first effect of the letters was that of a single complex impression, some characters appearing distinctly outlined, some confused, and some entirely unseen. This conscious impression followed the exposure in much the same way that a positive after-image follows a stimulation of light. It was sometimes possible to hold this impression with all its details an appreciable length of time without recognizing a single letter, until each character was recognized one at a time. But it was more frequent that one or sometimes two letters were recognized without being preceded by an appreciable interval, and these followed by one or two more, one at a time and in distinct succession.

The letters that came up last were nearly always less distinct, although it was sometimes the case that the order was not the order of distinctness. A special effort to recognize an indistinct character would frequently cause it to mature into complete recognition before others which were at first more distinct. Sometimes a delay in this maturing process would cause more distinct characters to be forgotten before they could be named, or else cause the indistinct one to come floating into the mind as an after-thought when all had been given that could at first be remembered. This indistinctness of the letters recalled last is a comment upon the common experience that the impression as it is first received rapidly fades. One grasps at the most distinct characters in order to secure them before they fade, but with the feeling that in doing so he excludes the possibility of catching others which he might have taken in their place. The naming of the letters makes them seem more sure, and this is hurried up in order to get as many as possible. One subject noted that the delay caused by locating each letter in the field as it was recognized caused fewer to be got than when they were all named at first and then located.

The order in which the letters matured seem to be in no way dependent upon the order of exposure. In general, those in the middle of the field of vision, and hence seen most distinctly, were given first. It was frequently noted that some factor, other than the distance from the center of the field and the occasional voluntary effort, influenced the order in which

the letters were perceived. By referring the letters as given to the cards, it was found that the prevailing order was from left to right, as in reading; and this was as true of the reversed order of exposure as of the other. The habit derived from reading thus seems to have influenced the order to some extent. But no distinct influence seems to have been exerted by the reversing of the shutter.

Frequently the same letters would be given from a card for several exposures, and these would have their order varied independently of the direction in which the shutter moved. The letters in the center had the preference, while those sometimes upon one side and sometimes upon another would be given with these, a fact which would seem to indicate that the attention before the exposure wandered about the fixation point rather than distributed itself equally over the field, as was also the case in the reaction experiment with concentrated and attempted distribution of attention. This probably explains another experience which might be interpreted in favor of a distributed attention and which was common to the subjects. When several letters were seen they were less distinct than when only one was got. This is in line with the law above quoted from Hamilton and illustrated by Wundt,<sup>1</sup> that 'the greater the number of objects to which our consciousness is simultaneously extended, the smaller is the intensity with which it is able to consider each.' A comparatively large number of letters was commonly got as the result of a special effort of attention. Supposing this effort to cause the attention to fluctuate more rapidly about the field, a noticeable result of a special effort at distribution, and this to be correlated physiologically with a correspondingly intermittent rapid central stimulation of the visual centers, we can see that these centers would, as a whole, be kept in a more responsive state than when less effort was made, and the fluctuations were slower. This would cause the getting of a larger number with the greater effort. We can also see that with the less effort and slower fluctuation the center stimulated at the instant of exposure would reach a more highly excited state because the fluctuation was slower and the

<sup>1</sup> 'Physiologische Psychologie,' II., S. 268.

time of stimulation of the center longer. Hence the greater vividness of the single letter and the less vividness of the several. Thus this experience may be explained without the aid of distribution.

The fact that the letters in this experiment were not perceived or made conscious until after the exposure was over, separates the time after the instant of exposure into the two natural divisions, one a period of inertia, or subconscious period, and the other the conscious period.

#### 8. THE SUBCONSCIOUS PERIOD.

When the eye is stimulated we have, first, a so-called latent period of variable duration during which no effect of stimulation is shown. This is founded primarily upon analogy with the general functioning of the nervous system, since electric stimulations applied to a nerve do not cause an immediate muscular contraction. This period is very short. Second, there is a very brief but relatively longer period during which the effects of stimulation reach a maximum. This is illustrated by 'recurrent images,' or the 'oscillatory' activity of the retina. It is shown when a black disk, illuminated brightly by sunlight, and containing a white sector, is rotated at the rate of about one revolution in two seconds. With the eyes fixated upon the center of rotation, the sector seems to have a shadow upon it a short distance behind the advancing border, and this may be followed by a second fainter one, and even by a third still fainter. The distance between these, and between the first and the forward edge corresponds to a time period of about .015 of a sec. "It thus appears that when light is suddenly thrown upon the retina, the sensation does not at once rise to its maximum, but reaches this point by a sort of vibratory movement."<sup>1</sup>

In addition to this inertia of the retina there is also inertia of a more central origin. By means of experiments in reaction time, Cattell found 119σ and 116σ as the time necessary for two subjects to distinguish one capital letter from all the others when the letters were the size of the capital of an ordinarily printed

<sup>1</sup> Bowditch, in 'American Text-book of Physiology,' p. 790.

page. With the same subjects he found the whole reaction time which included this process to be  $308\sigma$  and  $324\sigma$  respectively. It took longer for some letters than for others to the extent of some  $20\sigma$ .<sup>1</sup>

This period of inertia preceded the completed act of perception in our experiment, and hence errors in the process are traceable to it. Thus it was a common occurrence for one letter to be taken for another which largely resembled it. *C* and *G* were often confused, also *V* and *X*, *O* and *U*; here, no doubt, the part of one letter only was seen which resembled a part of the other, and the rest was filled out wrongly as in an illusion. More striking were the cases in which letters placed far apart on the card would be given a place half way between with the position seemingly sure, but with uncertainty as to which of the two letters it was. An *F* might be placed far from its real position and beside a *C* without the latter letter having been seen. A *Z* standing by itself was given as *Z* and *T* close beside each other. *G* was interpreted as *G* and *C* beside each other and in the place of an *I* which was not seen. Letters are made up of different parts or elements of form in the same way that words are made up of letters. Such cases as the above indicate that elements of form and position were received as disconnected impressions at the instant of exposure, and that while these in the majority of cases were correlated subconsciously into the right letters in their right places, they were yet often combined into wrong letters and in wrong places. It was not, however, until this correlation was completed at the end of the exposure that the letters reached their first conscious stage, and it was possible for the attention to be divided among them. So that if distribution can take place in tachistoscopic experiments, it is only when the exposure is over, or when it is longer at least than the  $20\sigma$  exposure of this experiment.

In this connection it is important to know how long a time can be taken up by the consecutive exposure of letters and still have them seem to appear simultaneously. It was found that

<sup>1</sup> 'The Time Taken up by Mental Operations,' *Mind*, XI. (1886), p. 220.

For discussions of inertia see Wundt, 'Phys. Psych.,' I., 321 ff.; Fechner, 'Psychophys.,' II., 431 ff.; Exner, 'Herman's Phys.,' II., 215; *Pflüger's Archiv*, XXVIII., 487; Hofbauer, *ibid.*, LXVIII., 546.



the rate of the shutter could not be varied sufficiently for this purpose with the rubber band, so that a weight attached to a cord running over a pulley and attaching to the shutter was substituted for it. The slowest rate which could be attained by this means was produced by allowing the shutter to be drawn by the falling of the weight when it started from a position of rest. More rapid rates were produced by raising the weight to various heights before releasing it, so that a degree of velocity was attained before it reached the point in its fall at which the shutter was moved. The extremes of slow and rapid rate procured in this way were much greater than were needed by the experiment. The height from which the weight fell was recorded, and the exposure varied several times from both the too-rapid and the too-slow rates to the point at which succession was just indiscernible. When this point had been determined for each subject the time of the exposure was ascertained by means of the tuning-fork and smoked paper method previously described. The following table gives the time of exposure in thousandths

TABLE V.

Subject.	<i>Am.</i>	<i>An.</i>	<i>Ho.</i>	<i>Hu.</i>	<i>M.</i>	<i>R.</i>	<i>S.</i>	<i>Y.</i>	Av.
Time.	26	24	86	34	75	75	27	28	47

of seconds in which the sequence was just indiscernible for each of eight subjects. The average rate thus obtained for the whole exposure was 47  $\sigma$ . This average is of little value because of the wide differences of rate found for the different subjects. In this test the shutter moved from right to left. When the exposure was too short for succession to be noticed, the order in which the letters came to consciousness was from left to right, as in reading. With all of the subjects except *Am* and *Hu* it was noticed that the first sign that the objective order had become apparent was the tendency to give the letters from right to left in the order of their exposure. With *Am* and *Hu* the movement of the shutter was first perceived as such.

What causes this lack in the perception of succession? At first we might think of it in connection with the after-effect of retinal stimulation which makes color mixing possible with the



color wheel. The duration of this varies from .100 to .033 sec. according to the intensity of illumination, the length of the stimulation and the color. The difficulty with this solution consists in the fact that while in color mixing the same part of the retina is affected by the different colors, in the above test, different parts were affected in succession as the exposure took place. The divergence of these parts was about 0.97 mm. when one of 0.004 to 0.006 mm. is sufficient to distinguish two adjacent objects. In this experiment the breadth of the exposed field was 6.3 cm. and the distance of the eyes of the subject from the apparatus 1 meter, with the exception of *M* and *R*, for whom it was 60 cm. Even though the after-effect of the part of the retina affected first should continue until after the stimulation of the part affected last took place, this, it would seem, need not prevent the initial impulses from being felt separately. This suggests that somewhere on the route of transmission the visual impulses from different parts of the retina traverse paths held sufficiently common to cause the impulses received first to overcome inertia, thus allowing the later impulses to overtake the first and so reach the center of consciousness nearly or quite simultaneously with them. On the other hand, it may be that the stimulations which come in succession from different parts of the visual field are transmitted immediately to the cortex. A certain time interval between the central nervous impulses thus aroused might be necessary in order for them to remain sufficiently distinct to mediate discrete sensations.

(To be concluded.) 1198

## DISCUSSION.

### IMITATION AND SELECTIVE THINKING.<sup>1</sup>

My original difference from Professor Baldwin was merely about the adequacy of 'imitation' as compared with 'identity and difference' in explanation of social processes. But, through the discussion of selective thinking as the instrument of the genesis of organization out of imitation (I am recapitulating roughly and without controversial intention), we have now got on our hands the value of selective thinking. And I am not sure that this does not open up the whole subject of Pragmatism. I say I am not sure, because this depends on the limit which Professor Baldwin puts to the meaning of his term 'genetic.' I will, if I may this once more trespass on the courtesy of the *PSYCHOLOGICAL REVIEW*, try to distinguish my attitude in respect of the different questions thus raised, mentioning Pragmatism so far as to show what I conceive would be the problem evoked by it, in contradistinction to those to which a genetic theory as such ought, in my judgment, to confine itself.

1. As to Imitation *vs.* 'Identity and Difference.' *a.* I can not agree with the view that this difference is verbal, *e. g.*, in its application to biology. It is easy to show that a working whole cannot be represented in terms of similarities between its members. And for the same reason the nature of the members themselves must in a great measure be omitted from such a representation. The Linnean classification, or the current 'natural' classification in botany, may be taken as a representation according to resemblances, though I should not admit that any scientific classification is so intended. But a region of the world, as a whole of competing and coöperating members, according to the light thrown by the principle of evolution, can never be represented in such a form as this. It can never bring together the things which have most to do with each other: competing species of plants, coöperative plants and animals, the soil, the climate, and their effect on the living things. Of course all this can be added in footnotes, as it were, to classification by resemblances; but it cannot be represented in the structure of the classification itself. It would be like trying to explain

<sup>1</sup> Continuation of the discussion in earlier numbers, with especial reference to Professor Baldwin's paper in the issue of January, 1903, p. 51.

a locomotive by arranging its parts in classes according as they resemble each other. The reason of the impossibility is that the parts or members have their connection through their differences; and in a classification by resemblances, these, though they have their weight as differences, have no weight as instruments of identity. This whole subject is treated by Green, 'Works,' II., 285, 'On Mill's Induction,' and I think is too little understood. I should strongly suspect that the reform of logic in this sense in the great Idealist days promoted, or at least was akin to, the transition from Linnæus to Darwin.

β. Imitation (I summarize in my own language) is alleged to be a *vera causa*, it *shows*, is psychical, genetic, you can see it at work; the operation of a universal is an assumption, shadowy, almost, I think, *a priori*, mystic, antiquated, invisible. I assume publicity, which ought to be explained.

Now I am very likely quite wrong, but I cannot see any ground for all this in the facts. Imitation no doubt is a fact, and plays an important part in furnishing the self with material. I quite recognize the value of Professor Baldwin's actual work at this point. But surely response and reaction, indices of communication through a common nature, are much wider and more primary facts, extending over the whole world, physical and psychical. The adapted response is earlier — is it not? — than consciousness; and the process of its passing under the control of intelligence and being emancipated from trial and error, is fairly well understood, though still doubtful in some details. But the adapted response, as controlled by intelligence, just means a consciousness of the situation based on an inference which *pro tanto* dispenses with the test of material action; an inference based on perception is substituted for a certain number of errors, as when a man sees at a glance how to open a gate, which a dog will paw at till it comes open. There seems to me no assumption in this; it is a plain statement of fact, and of fact more general and fundamental than imitation, and requiring no more assumption.

With responses adapted by intelligence on the part of two or more agents you have publicity or the situation. What you want, to account for this, is not imitation but the power of consciousness to combine perceptions and see their results — in short, the unity of consciousness. As I understand, it is urged that this must not be assumed but can be and ought to be genetically accounted for. This I will speak of when I come to comment on the meaning of the term 'genetic.' My present point is merely that imitation is the secondary, less general, and less completely stated fact, and that the assump-

tion of it, while involving, as much as a response does, the assumption of the unity of consciousness, is in no special way a help towards explaining the apprehension of a situation as a whole.

γ. The treatment of facts introduced by this theory, in the extension claimed for it by Professor Baldwin and others, seems to me precarious all round. Particularly is this the case with the separation of the imitator and the inventor. I am convinced that a really critical study of any branch of history would demonstrate the crudeness of this antithesis when offered as a matter of principle. The advance of the human mind, independently, so far as can be judged, of individual original genius, is one of the most striking phenomena of history, and one is inclined to add that the deepest transformations are those which have taken place in this way. It is an old and true saying that man must advance or recede; to stand still is impossible for him. That is to say, the application of tradition to life is in itself a generator of inventions; it is impossible even to borrow ideas without drawing conclusions which the lender never drew. And it is well known how rarely, if ever at all, an invention can be assigned to a single mind. The history of art is very instructive on this point, *e. g.*, the education of a Turner.

2. The second question, as it has now opened itself up, seems to amount to this: Does a genetic account of thinking explain by what character judgments are true, or only under what influence we have come in fact to hold (often wrongly) certain judgments to be true? And what bearing has either alternative on the theory of selective thinking?

α. I will say at once that I see no meaning in a genetic account of knowledge, except as a history of opinion; but I admit that this involves a history of mental organization. A simple illustration will do as well as an ambitious one. We constantly make such judgments as this: "A. B. is a moderate Evangelical; he was brought up as an extreme one, in a family and circle whose views were extreme, but his work and intercourse with varieties of people have made him much more temperate." Here we have the true place of a selective theory of thinking, so far as I understand it, in a nutshell. A. B. inherited a platform, an organized mental constitution and logical or quasi-logical system; *i. e.*, he acquired it by adaptation to his parents' and teachers' views, or imitated them. Starting from this, he developed his later position through varied forms of social selection acting on his ideas, involving accommodation to practical needs; and he now has a mental content and organization at once fairly harmonious with the

circle in which at present he moves, and determined as a whole by the platform which he inherited. I do not doubt for a moment that a history of all of us and of the human race could be written in terms analogous to these with a great deal of truth. And it would not omit the facts of mental organization. The metaphysician, the psychologist, the biologist, mathematician, and also the Englishman, Frenchman and German, would all prove to possess, yes, and to have acquired and developed, certain favorite categories, certain forms of logical or quasi-logical bias, and predispositions to accept explanations of certain appropriate types.

In such a historical enquiry some theory of selective thinking might have, so far as I see, very interesting applications. It would show by what needs and under what direction of attention the minds of nations and individuals had grown into certain structures, and had acquired certain logical predispositions.

But even here it would be necessary either to expand very largely the sense in which, or to limit very strictly the extent to which, we affirmed action to be the instrument of selection. If action meant all psychical change directed to an end, then, in referring the course of cognition and mental organization to the needs of action, we should be making cognition itself the standard of cognition,<sup>1</sup> and saying that it learns to act as it does act primarily by seeking its own ends and secondarily by taking account of a certain contact with material action. Then we might fearlessly say that 'action' is the sole test and instrument of selective thinking. How 'action' operates, would be the further question, to which Logic would be the answer.

<sup>1</sup> Mr. Stout in his 'Manual of Psychology' seems to me to agree much more with me than with Professor Baldwin, never blinking the relative importance of the cognitive system as compared with external action, nor the liability of social endorsement to be erroneous. But in one place he seems for a moment, as I venture to think, to slur the distinction on which I am here insisting. On p. 547 he insists that because belief is a condition of activity, therefore activity must be a condition of belief. And this remark he extends to theoretical activity, though, indeed, as referring it to the provisional acceptance of working hypotheses, he gives it a very restricted and innocent application. But the point I wish to urge is this. In a 'practical' activity the end is assumed to be given, and it is not a cognitive end; therefore in this case there is some tendency to adopt beliefs which purely cognitive processes might not confirm; *i. e.* there is a possibility of a real non-cognitive influence on cognition. But in a theoretical activity, unless a preconceived opinion is to be supported (which is an *aberration from the theoretical consciousness*), the end to be obtained is not given, but is itself a conclusion to be constructed. It therefore involves *ipso facto* a modification of the beliefs ancillary to it, and the dangerous primacy of action over reason is not confirmed by this instance.



If, on the other hand, action were taken in the sense of the production of change in the external world, we should return quite a different answer. We should say that the influence of practical needs was a diminishing factor as the content of systematic knowledge increased.<sup>1</sup> We should point out that when thought has become complex, action on the external world is to it as sensation is to science, a condition which is little more than negative; something, disagreement with which demands more or less modification of the discrepant thought, but any given agreement with which carries us but a very little way towards truth. We should further urge that the much talked of 'social endorsement,' as applied to systematic ideas, has no existence. This is a very important point in its practical bearing. Social endorsement does apply roughly to habits of action. But to cognitive ideas, to the actual content of inventions, and to theories, as such, it has no application, only touching them in one or two points out of thousands; and to suppose otherwise is a very mischievous superstition.<sup>2</sup> It is a transference of the ideal postulate of reason, that all valid judgment is valid for all intelligence, to the *de facto* social consciousness, to which it applies only in grades so contingent and varying as to be of no selective value whatever. The leading ideas of society, so far as they can be conjectured from their expression, are always in arrear of the truth known to experts, and more especially are discrepant with its own habits of action, which do represent in a rough and unorganized form the external needs of life.<sup>3</sup>

The exclusive importance attached to action on the external world, and to social endorsement, even as influences on the history of opinion, is, I hold, a mere paradox, unsupported by facts. The subordination of the vast cognitive systems and interests of mankind (which have, it must be remembered, their own relations, dictated by cognitive needs, with the 'external world' or sense-perception) to the test of action in the narrower sense of material external change, I believe to

<sup>1</sup> Mr. Stout in his *Manual* seems to me perfectly clear on this point; and to be wholly free from the ambiguity whether thought is made true by being socially and practically selected, which I find in Professor Baldwin.

<sup>2</sup> I hope I shall not annoy a friend who conversed with me in the U. S. A., in 1892, if I make use of his observation to me: 'Sir, the people of these States have endorsed the philosophy of Mr. Herbert Spencer.' The example seemed too apposite to be neglected, as showing the laxity with which a rough coincidence in one or two points is construed as an 'endorsement.'

<sup>3</sup> *E. g.*, T. H. Green usually agreed with J. S. Mill on questions of public policy, though on all theoretical matters their minds were diametrically opposed. This is possible, just because theoretical ideas, even of social matters have so very little of their content in contact with practice.

be simply an elementary blunder. If, on the other hand, we are only asked to call these interests and systems 'practical,' as Aristotle carefully pointed out that they are, in virtue of their inherent conativeness, we are asserting, I take it, the contradictory of Pragmatism,<sup>4</sup> but are returning to obvious truths. (See Stewart, 'Notes on Aristotle's Ethics,' 1098, a 3, and citations from the 'Politics'.)

β. And when we raise the whole question of Pragmatism, *i. e.*, as I understand, not '*How do we come to think something?*' but '*What is the test of its truth?*' the idea of selection by social endorsement, or by success in producing change in the external world, loses all claim to consideration, except as involving agreement with sense perception, which is provided by cognitive activities in a much more adequate form. As we have seen, nearly the whole of cognition is simply untouched by action on the external world. In such action itself the outward change effected is but a minor part, from which, as we know, *e. g.*, in all ethical considerations, it is impossible with certainty to understand a man's mind; and when we come to the great cognitive systems the prerogative of such action vanishes altogether. Indeed, there is but one criterion of truth, and that is, a fuller systematic cognition of the content whose truth is in question. No history of opinion, no formation of a platform, no idiosyncrasies of mental organization, can come into court when the question of truth is raised. Then we have to do with nothing but the systematic necessity of knowledge and the fact that fuller cognition can compel every false judgment to expose itself as flat self-contradiction.

I do not feel sure whether these remarks are relevant to Professor Baldwin's views. But he seems to me to mean that selection by social and practical needs not merely accounts for our holding opinions, but also constitutes their truth or falsehood. If so, then, as I said at starting, we have the whole of Pragmatism on our hands, and are, as I hold, beyond the limits of legitimate genetic explanation. Grant, *e. g.*, for the sake of argument, that the unity of consciousness first appeared in practical action in the narrower sense given above (as it must have done if there was a time when consciousness was entirely 'practical' in its aim), or that it is motor in its nature, or that it appears in some sort of general sensory process. All that is interesting in the history of opinion, but has no bearing on the logical value of

<sup>4</sup> Because Pragmatism says, as I understand, that the only ends of action are those which consist in change wrought upon the external world, and that, to these, cognition is a means. For me, cognition, as a harmony in our experience, has the character of an end of action, though not the whole end. But external change is *never* an end.

such unity. This is only to be discovered by an analysis of the part played by it in the organization of experience so as to avoid self-annihilation by self-contradiction. It is an old story; granting (what is not true) that we need not play the game, yet if we sit down to it we must observe the rules. If we are asked, Why must we? there is no answer but to show by analysis in any given case that in trying to evade them we are disguisedly throwing up our hand. I can imagine its being replied, 'But you say that A.B.'s rules and platform are got by his history and education; then surely his truth is so too.' The answer is that his rules and platform are an imperfect appreciation of *the* rules and platform, and cannot stand against another, in him or outside him, which more nearly approaches them, and therefore is able to exhibit his as self-contradictory. His knowledge, or rather opinion, *qua* his, may be compared to his body, a *de facto* structure, accounted for by accident and selection as well as nutrition and correlation. But his knowledge *qua* knowledge may be compared with the work his body is now capable of as a machine—a test to which his genesis has nothing whatever to say. Truth is the most organized organization of reality in the medium of judgment; our history may excuse our failures in it, but cannot make them successes.

It does seem odd to me that views like this should suggest to any one the idea of 'the mind, for no reason, and by no regular processes, making its truth what it will'; or of 'the essential mysticism of *a priori* formalism which prevailed before the rise of the genetic point of view'; or the assertion of determinate variation<sup>1</sup> in the narrow sense that no natural reason can be given for whatever balance of variation may be found in a given direction.

This again is an old story. The very error with which I am charged appears to me to be merely in the mind of my antagonist. The whole antagonism of principle between classical and modern

<sup>1</sup> Perhaps my terminology in speaking of determinate variations was incorrect according to biological usage. I meant by determinate variations those which by a definite cause are produced in a definite direction, which is such as to be appropriate to the environment, so that subsequent selection becomes needless or inoperative. This, it will be seen, is exactly the opposite of the meaning which my statement conveyed to Professor Baldwin. If I am understood I do not care to argue about the true biological conception; but I am disposed to think that this will soon prove less formal and more real than Professor Baldwin supposes. What is meant by speaking of a principle of determination that does not show itself in any recognizable or conscious process I cannot form any conjecture. Is it not a recognizable and conscious process by which we determine that the three angles of a triangle are together equal to two right angles?

logic; the whole conception of a modern development of the genetic point of view, considered as anything which affects the nature and criterion of truth; the whole idea of thought in itself as opposed to the nature of the real in cognition, all this appears to me to be the merest mare's nest. The truth of anything is for me simply its fullest nature so far as expressible in judgment, organized, as the fullest nature must be, so as to avoid diminution by the contradiction of its parts. What I deny is, not that thought is the expression of organized reality, but that the organization of reality is confined to the production of material change in things. The nature of things is both general and special, and besides its more general and formal characteristics, there are all sorts of grades and variations as we push deeper and deeper into the heart of complex individuality. These, as found by analysis, form respectively the more abstract and more concrete elements of Logic. But obviously all of them contain and confirm the general nature of truth.

Why should not the universal 'be a mental experience which has for its physical counterpart the synergy of adapted action' (p. 63)? To me the answer seems simple — because there is very little thought, proportionately speaking, to which there is any adapted action, *in the sense of external material change*, to correspond. I have said that I think that unity very likely first showed itself in adapted action. But no thought, probably, ever had its content exhausted in the adaptation of external action; no thought of a cultured mind can ever be so exhausted to-day, even in the most practical of activities; and a very great part of life, a part which even economically and industrially is an immense and commanding interest in the world, has no end in external adapted action at all, but on the contrary uses and transforms such action by making it its means. A great scientific laboratory, for example, has not its unity in a material operation to be produced; its actions have their unity in a cognition to be attained. The same point is very strikingly shown in the enormous material activities of a Wagner or Handel festival; whose whole practical business has for its determining purpose the production of a harmony in minds, of the same *general* (not specific) nature as a cognitive state. The harmony is the end; the 'action' is the means.

The formation of new reality seems to me a contradiction in terms; but the discovery of reality new to us, and the adaptation of intelligence to it, is surely a fact which no one has ever denied, and which, in general, is hardly worth affirming. It is presupposed, for example, in all education; and the ancient educationists at least never imagined that

education was only for the young. Now if this distinction would satisfy the genetic point of view, I think we might come to terms. But if the genetic point of view means (*a*) that new reality is not merely discovered but created, (*b*) that action on the external world, and social selection, are the determinants and criteria of truth, then I fear there can be no truce between us.

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I venture to append here a word of reply to Professor Bosanquet, since as editor of the *REVIEW* I am afraid I shall not be able to take further liberties with the space available for discussion in later numbers. The remarks which follow refer by number to his divisions above. The discussion has served a good purpose, let us hope; and readers of the *REVIEW* will always be ready to follow Dr. Bosanquet in the further development of his views in these pages.

*Introductory remarks, and 2β.* — I do not intend to raise the question of pragmatism as a philosophy, in advocating a theory of selective thinking by active adjustment processes. Personally I am not a pragmatist for much the same reason, as I conceive, that Dr. Peirce (the originator of a certain form of that view) is no longer one (cf. Peirce, art. 'Pragmatism' in the writer's *Dict. of Philos.*, vol. ii.). I think pragmatism is not able as such to explain the general or 'universal' aspects of reality. I agree, therefore, with Dr. Bosanquet in confining genetic theory strictly to questions of genesis. But — and here possibly the final issue between his views and my own may stand for others also to divide upon — *but*, there can be no truth nor cognized reality which is not what the human mind has reason for accepting and believing; and the reason always is and must be that this aspect of reality, now called true, stands or has stood the tests of certain selective processes. Genetic theory, therefore, explains *both* 'under what influence we have come to hold (often wrongly) certain judgments to be true' and also 'by what character judgments are true' (2). It explains just the character which means to us truth — reality as postulated by thought. All such antitheses, accordingly, as that quoted above — that between what is true and what seems true — are radically false antitheses. It is the business of theory of knowledge to establish the 'is' in and upon the 'seems.' Now it is the claim of the theory of social selection that by it certain of these antitheses are banished. What 'seems' to the individual is true only relatively to the larger 'seems' of the social group; what seems to the evangelical is true, but relatively to an enlarged socially established creed, etc.



Otherwise we must go back to the traditional correspondence theory of truth—a dualism involving separate and static realities set over against a series of mental images which only partly correspond with them. Dr. Bosanquet seems not to hold such a dualism; indeed he defines truth as ‘the most organized organization of reality’ (2a). This definition I adopt provided ‘reality’ be defined (so far as cognized) as nothing more than the human system of truths. But I fancy this will not be acceptable to Dr. Bosanquet, for it then makes a tautology which can be resolved only by a theory of progressive organization of richer truths (and realities) on the basis of less rich—the whole by a process of selection.<sup>1</sup>

I believe, therefore (to answer the enquiry raised by Dr. Bosanquet), that reality is dynamic and progressive (just because truth of which it is a function—from which it is generalized—is such); that new realities are made on the process of human experience and discovery (for just by getting into the system of intelligent or other constructive experience, they are constituted as realities); that the phenomenal manifestations of change, of which time is the generalized mode, are the data of all real construction both cognitive and other.

With so much general statement I may take up very concisely certain of Dr. Bosanquet’s more detailed positions.

‘Imitation *vs.* Identity and Difference’ (1, a). I subscribe to most of Dr. Bosanquet’s remarks under this head; indeed I think ‘imitation plus invention’ is very fairly described as ‘identity and difference.’ The barrenness of the formula for the progress of science is what I should still claim. Certainly identity (the whole) realized in differences (the parts) is the ideal of knowledge, but empirical science proceeds by generalization, classification, etc., in constructing its experimental wholes.

1,  $\beta$ . The fallacy, as I take it, of Dr. Bosanquet’s criticism of the imitation theory here resides in the assumptions: (1) that *adapted* action is the same as *adaptive* action. Certainly adapted action always exists, imitation is itself a case of it (I think, broadly defined as ‘circular response,’ the original case of it); but the question is *whence and how is action adaptive? How are new adjustments effected?* It is no answer to say it ‘passes under the control of intelligence,’ and is thereby ‘emancipated from trial and error.’ For such control must be either by the using of old adaptations simply or by

<sup>1</sup> The criticism of pragmatism as a philosophy would raise the question as to what place such a system of cognized realities has in the total system of our whole experience of reality.

the establishing of new ones. If the former, then there is no progress;<sup>1</sup> if the latter, then again — *how is it done?* No one would (or should!) claim that the intelligence works by saying to the contents of the mind, Do this — and it doeth it! It does it *by the process of trial and error*, instead of being emancipated from that process! And it is, I think, a fairer inference to conclude that, each new adjustment being acquired in this way, then the presumption is that all the old ones were also so acquired. Applying this to social life — the trial and error process *is imitation*.

2. A second assumption is that if two minds work similarly they will be *ipso facto* in social relation with each other in a situation recognized by each ( $\alpha, \beta$ ). The need of 'social endorsement,' imitatively secured, as applied to 'cognitive ideas, the actual content of inventions, and to theories, as such' is declared to have 'no existence' and to be a 'mischievous superstition.' This is the reply to the 'sheer mysticism' by which I characterized the opposite view — the view which flatly assumes 'logical' process. But on this I can not retract. Dr. Bosanquet says: 'It is the transference of the ideal postulate of reason, that all valid judgment is valid for all intelligence to the *de facto* social consciousness.'

It is! — not indeed to the *de facto* social consciousness, but to the developed social consciousness *as reflected*<sup>2</sup> in the judgment of the thinking individual. Otherwise must we assume an individual essentially unindebted to his kind so far as his logical competency is concerned, a society working by a sort of 'preëstablished harmony' among its members, and something like Rousseau's 'general will' to formulate the most striking facts of social life. Instead of an 'ideal postulate' this, to me, is a 'postulate of the idealists' (of a certain type). It is logical formalism — and that smacks of mysticism.<sup>3</sup> Thinking has this postulate indeed; but how it gets it — that is the

<sup>1</sup> This is the case with calculation, deduction, etc., which show simply the mind running along its old established mental habits (cf. my earlier reply to Dr. Bosanquet, in the issue of January, 1903). You can't make this sort of logical process ultimate by simply saying that it exists, or that, 'we have to do with nothing but the systematic necessity of knowledge'; indeed Dr. Bosanquet's criterion of truth, the 'fuller systematic cognition of the content,' itself raises the question as to how the cognition can become fuller.

<sup>2</sup> Both by his heredity from ancestors all the while socially selected and also by his extended social education. This account of, rather than denial of, the individuals' essential initiative and self-determination seems to escape Dr. Bosanquet's notice. (Cf. *Soc. and Eth. Int.*, 3 ed., Chap. VII., VIII.)

<sup>3</sup> Something of what I mean by mysticism is seen in Dr. Bosanquet's chapter on the 'General Will' in the *Philosophical Theory of the State*.

question. I think it is one of the characters it owes to selective processes. Only by imitative social experiences can there arise the *identity of content* requisite both for social evolution and also for the individuals' integration in a 'public' situation. Mere identity of logical function — even though it be assumed — would not be sufficient.

1, γ. The 'separation of the imitator and the inventor.' This criticism seems beside the mark. So far from separating the two, I make all invention variation in the imitative processes. There is no mere imitator save the parrot; and there is no pure inventor, save the crank — and neither of these really is!

2, α. Here Dr. Bosanquet gives a good account of the more evident features of the theory of 'selective thinking' and then vitiates it by his definitions of 'action' as the instrument of selection. I do not accept either of the definitions he proposes; least of all is 'action' the 'production of change in the external world'; yet it is against this crude conception that most of his definite criticism is directed<sup>1</sup> (and I accordingly leave it unanswered). The other definition on which he would himself accept selective thinking is that action means 'psychical change directed to an end' — in other words, his 'logical process'.<sup>2</sup> No, I cannot accept that — although such is always present. But the new — the acquisition — is *ipso facto* in so far the unforeseen. It is what survives, from variation, after an intended organization of contents. It survives (is selected) under two tests: first, its possible organization with the individual's earlier systematized stuff of thought (a psychophysical matter of synergy of motor, largely attention, processes); and second, its relative adjustment in the environment of any sort — intellectual, social, physical, moral — in which the particular item makes a claim. This latter is enforced also largely through motor processes — by the inhibition of some conduct and the encouragement of other, as reflecting thought which suit or inflict those (in the social case) who are appealed to. No truth is established *for the first time* apart from adjustment to an external (material), social, or some other non-private world.<sup>3</sup>

In conclusion, it appears to me that Dr. Bosanquet's criticisms, made from the point of view of 'logical process,' lose point for the following reasons:

<sup>1</sup> Unless by 'external' he means unindividual, not private; in that case my reply follows.

<sup>2</sup> "How 'action' operates would be the further question, to which 'logic' would be the answer" (2, α).

<sup>3</sup> These selective tests are worked out in detail in the chapter 'Selective Thinking' in the volume *Development and Evolution*.

1. He does not allow that logical process in the individual may not only not forbid, but may require, selective processes operative in the imitative and social functions.

2. He does not admit that social progress in the race may be determined in the individual (by whatever evolutionary method) as ideal or other 'postulates of thought.'

3. He does not admit that systematic thought, under whatever adequate genetic theory, may not only discover but actually *constitute* new phases of cognized reality (or any tenable definition of that term).

4. He does not distinguish between a pragmatic (selective) theory of knowledge, and a philosophy which makes pragmatic criteria the complete final tests of metaphysical reality.

I may finally thank Dr. Bosanquet for the instruction I have derived from this discussion. Whatever 'inventions' have come out of it — they would seem not to have been reached by 'imitative' processes!

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#### NOTES ON DURATION AS AN ATTRIBUTE OF SENSATIONS.

The relation of any mental process to duration may be conceived in four different ways. In the first place, such a process may occupy a certain period of what we may call objective time, quite apart from any corresponding consciousness of its duration. To the monk in the much-quoted fable, his absorption in the bird's song seemed a matter of moments; its duration in the sense of objective time was a thousand years. Secondly, a mental process may possess duration for the consciousness in which it occurs. Thirdly, it may represent, or be an idea of, a certain duration; as when I run over in a few minutes of objective time the events of yesterday. Or, fourthly, it may be an estimate, a measure of a certain present duration made by reference to accompanying mental processes; as, for instance, the judgments made in time-sense experiments.

In other words, the problem of duration as connected with conscious processes may be four-fold. It may concern the methods of measuring the objective duration of a conscious process; such is the task of reaction experiments. It may concern our simple consciousness of present duration; the difficulty of isolating this aspect of the matter we shall presently consider. It may be to study the conditions which enable an idea to represent a certain duration in past or future time; finding, for instance, that such a duration is overestimated if its

contents are various, underestimated if they are monotonous. When Professor Münsterberg says, 'Wir können die Vorstellung eines flüchtigen Momentes lange festhalten, der lange Dauer schnell gedanken,'<sup>1</sup> he is expressing the fact of the independence of the first and third problems; the objective duration, measurable by chronoscope, of the idea of a long duration may be short. Finally, our task may be to find the subjective factors upon which our estimates of present duration depend; this is undertaken by the more recent 'time-sense' experiments.

Discussions of the temporal aspect of consciousness, as they are to be found in psychological treatises or even in special researches, seem not always to observe these distinctions. The third case is easiest to discriminate from the rest; the conditions of practical life make the difference between present duration experienced and past or future duration ideated, sufficiently emphatic. In regard to the others, however, we find two opposite tendencies. On the one hand, the simple consciousness of a present duration is identified with the objective duration of a mental process, measurable by physical duration. And on the other hand, this simple consciousness is identified with subjective estimates or measurements of it. Both of these tendencies, though differing in direction, arise from the same source: the difficulty of thinking of duration except as measured in some way. It may be worth our while to seek for some hint of the reason for this. We do not find the same difficulty in the case of the quality of conscious states. We can consider the quality of a color sensation by itself without being immediately led into the 'How much?' attitude. Now the process of introspecting any mental state is almost inseparably connected with the process of discriminative judgment. But the qualities of conscious states are many and varied; the process of discrimination finds material in the purely qualitative aspect of the phenomenon. The temporal aspect, however, must be quantitatively judged if judged at all; hence introspection is identified with estimate. Precisely the same difficulty arises in the case of intensity and extensity; here, too, the only possible judgments are quantitative ones. Wundt's interpretation of Weber's Law is of course an insistence on the distinction between consciousness of intensity and subjective estimates of intensity. And confusion between consciousness and estimate was responsible for the fact that the Stumpf-James doctrine of 'crude [*i. e.*, unmeasured] extensity' won its way but slowly.

Along with this natural tendency to identify fact with estimate of fact, there coöperates in the case of duration another condition, which

<sup>1</sup> 'Psychologie,' I., S. 247.



leads in the direction of making that estimate a physical rather than a subjective one. As regards intensity and extensity such a confusion could not occur. It is easy to convince the plain man that physical measurements of stimulus, a mode of motion, cannot be identified with measurements of the intensity of sensation; while in the case of extensity he finds it clear that since the spatial relations of the stimulus are lost in the nervous process, and the area of brain surface does not represent that of the body, the spatial character of the mental process and that of stimulus, or excitation, must be disparate. But the case is different with duration: the duration of a physical process is supposed to be absolutely homogeneous with that of a mental process. They can be measured in precisely the same terms; that is, the objective duration of a mental process is measurable in the same terms as the duration of a physical process. Now, the objective duration of a mental process is not the same thing as the consciousness of its duration: their identification is much less justifiable than that of the consciousness with the subjective estimate of duration. But given the tendency to make simple consciousness equivalent to quantitative measure, we can see why in many cases the measure assumed has been one of objective duration of conscious processes, which bear so attractively simple a relation to the duration of the accompanying physical process.

Turning at length from these general considerations to a more special question, there is a difference of contemporary opinion as to whether the simple sensation shall be allowed a temporal attribute. For example, Wundt says: 'A sensation thought of by itself can no more have temporal than it can have spatial attributes.'<sup>1</sup> Münsterberg would not credit the psychic element as such with temporal 'form qualities.'<sup>2</sup> On the other hand, Külpe and Titchener assign duration as a property of all sensations, and Ziehen says: 'Each sensation has a definite duration, which in general corresponds to that of the stimulus,' and absolutely to the duration of the central excitation process.<sup>3</sup> One is naturally led to try and disentangle the real significance of these opposed views, and for this end the question first suggests itself as to what kind of relation between the mental element and duration, and what conception of the mental element itself, are assumed by the people who deny that sensations have duration. We mentioned four such possible relations at the outset; of these the third is naturally out of the question here. Nobody supposes that a simple

<sup>1</sup> 'Grundriss,' 4th ed., 183.

<sup>2</sup> Cf. 'Psychologie,' S. 289.

<sup>3</sup> P. 130.

sensation, though centrally excited, can be an idea of past or future duration. It is evident, from Wundt's statement, that the fourth kind of relation is the one he has in mind, and that he identifies consciousness of duration with its subjective estimate or measure. Immediately after the sentence just quoted from the 'Grundriss,' and by way of further expository comment on it, he says: 'Position in time can be possible only when single psychical elements enter into certain characteristic relations with other such elements.' Position in time is a quantitative determination, which seems here to be made equivalent to the temporal attribute of a sensation. The same conclusion is suggested in another passage, where he says: 'One time-idea may be more lasting than another, but no time-idea can have absolute duration, for without the double relation of different sensations to one another and to the ideating subject, no such ideas at all could arise.' 'Time-ideas' here must be estimates of time; and a time-idea that lasts longer than another must be a duration estimated as longer than another duration. It is evident that if duration is thus merged in estimate of duration, it cannot appertain to a mental element as such, for estimates can be made only by referring the estimated thing to something else. But why does not the same argument militate against making intensity a property of the mental element? Why should not intensity be identified with estimates of intensity? If it is, evidently the law of relativity demands that it appear only in connection with combinations of elements.

On the other hand, it is possible to deny not merely temporal modes and forms, quantitative determinations of the temporal attribute, to the mental element, but to refuse to admit even the first relation between sensation and time-attribute; one may deny that sensations possess objective duration. I do not refer here to Professor Münsterberg's much-discussed doctrine of the timelessness of the psychical, which seems to me to have been not always clearly apprehended by his critics. So far as I can at present understand that doctrine, he does not assert that mental processes have no objective duration, but only that they have none when considered apart from their accompanying physical processes. Objective duration is not confined to physical processes; it belongs to psycho-physical processes, although not to the purely psychic—which is a matter that need not concern the psychologist if his parallelism is thoroughgoing. But quite aside from this, and granting that mental processes in general possess objective duration, one may still deny that sensations possess it, if one regards sensations not as concrete processes but as purely abstract products of analysis,

'an ideal of analysis rather than an actually existing mental content'<sup>1</sup>, to quote Professor Witmer's words. If they are concrete processes, they must be assigned a certain objective duration in which to run their course.

If we now turn our attention to the people who allow the temporal attribute to sensations, and ask in what sense they understand the term duration, the answer is not far to seek: it is objective duration they mean. Külpe plainly says: "To determine the quality, intensity and spatial characteristics of sensation, we have to rely on the subjective methods of sensitivity and sensible discrimination. Duration, on the other hand, may be measured by objective procedure,"<sup>2</sup> and he proceeds to discuss the reaction and frequency methods, which measure no subjective characteristic of the mental process whatever. They determine, not how long it is to the experiencing subject, but how long it lasts as a psycho-physical phenomenon measured by objective standards. The strength of this position lies in the fact that, as we have just remarked, every concrete process must possess objective duration. But can objective duration be regarded as an attribute of a conscious state? Should not the attributes of a conscious state be the aspects which it reveals to the consciousness in which it occurs? Objective duration belongs to a mental process not as that process is an object to the mind immediately conscious of it, but as it is an object to some other mind; as it is objectively regarded. I may consider my friend's mental state to have had a duration of five minutes measured by a watch; for him, as he is subjectively experiencing it, its objective duration is not represented in any such terms, is not directly represented at all.

We may ask another question. A sensation is a simple mental process. If duration in the sense of filling objective time is one of its attributes, are we not to set any limit to the time it may endure and still be a single mental element? Would the tone sensation produced by a body vibrating continuously through five minutes be a mental element? Such a sensation might outlast the 'psychic present' and be apprehended not as a single, enduring moment, but as a succession of moments having similar content. Could we call such a process simple? And if the objective duration of a sensation may exceed the limits within which a sensation remains single and elementary, what are those limits? Should we say that the maximum objective duration of a sensation is that of a single apperceptive 'moment,' since a

<sup>1</sup> 'Analytic Psychology,' p. 226.

<sup>2</sup> 'Outlines,' pp. 379, 380.

sensation lasting through more than one is for consciousness rather a succession of similar contents than a single process? It seems to me that there are only two ways of avoiding such a conclusion. One is to declare that the sensation is not a concrete process at all, and has no objective duration, being merely a postulate of analysis; the other is to say that the duration attribute of sensation is a purely subjective duration consciousness, and not in any sense identical with that which is measurable by chronoscope and the 'frequency method.' After all, surely the attribute of a conscious process cannot be measured directly by objective means; it is quite as meaningless to say that a sensation attribute can be measured in thousandths of a second, as to say that another one can be measured in grams. The sensation itself as experienced knows no more of thousandths of a second than it does of chronoscope wheels.

But suppose, then, that we allow sensations a psychic duration, as distinguished from objective duration. Whatever the character of this consciousness, it must be contained in the 'psychic present,' else surely we are no longer dealing with a single element. Within this limit, then, sensations must be capable of differing in subjective duration. But can the 'psychic present' have subjective duration? It may have a varying objective duration, naturally, and this objective duration may be estimated in time sense experiments by being inferred from certain accompanying phenomena. But is not the subjective present, as such, without subjective duration? This is the thought expressed by Volkmann, as follows: "In der Vorstellung der Gegenwart als solche ist die Dauer nicht eingeschlossen, denn mag auch die Gegenwart andauern, wir werden uns dieser Dauer darum doch nicht schon unmittelbar bewusst, denn auf die Zeitfolge bezogen ist die Gegenwart nur der absolute Mangel jeder Zeitbestimmung." He adds a sentence which sharply distinguishes between subjective and objective time: "So wenig wir die Vorstellung der Folge haben, weil wir aufeinander folgende Vorstellungen haben, so wenig werden wir uns die Zeitdauer schon dadurch bewusst, dass eine Vorstellung andauert."<sup>1</sup> It is a corollary from this view that the psychologically primitive time judgment is not one of duration but of succession. The psychic moments follow each other; they have no duration. The perception of duration is the perception of two or more such successive moments having similar contents.

It is possible, then, to deny on three different grounds that sensations possess duration: First, because their duration cannot be sub-

<sup>1</sup> 'Psychologie,' II., S. 20.

jectively estimated or measured without comparing them with other mental processes. This view identifies fact with quantitative estimate of fact; and it would surely be equally possible to deny sensations intensity, since one intensity can be estimated only in relation to others. Second, because sensations are held to be mere abstractions, not concrete processes, and therefore possess neither subjective nor objective duration. Third, because it is held that a sensation which lasts, objectively, longer than the psychic present is not a single element, while one that lies within the psychic present has subjectively no duration. While it seems absurd to treat their objective duration as one of the attributes of sensations, yet the only other way to maintain that they have a temporal attribute is to hold that the subjective present does possess subjective duration, a view which I think introspection does not confirm.

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#### IMAGERY.

While it is generally recognized that in psychological experiments great importance attaches to the determination of the general sense imagery type of the subject or observer, it is not yet proved that a subject is always of the same type. That is, he might possibly be of one type in one set of experiments and of another type in another set. Or if it be objected that a type in this sense is too stable to suppose such changes to be possible, it is yet conceivable that an individual may employ at one time say in one set of experiments one kind of supraliminal<sup>1</sup> images, and in a set of experiments with conditions changed another kind of images. Or it may be that, for certain researches, one type of person is preferable to another. Thus, for example, in the research of Miss M. L. Nelson (*Psych. Rev.*, Sept., 1902) on the 'Visual Estimate of Time,' the irregularity of the results may be explained by the fact that her subjects were not of the right type or that their type was not fully determined or kept constant. The subjects in this research were told to compare empty time end-marked by

<sup>1</sup> In order to make myself as clear as possible I shall use the term *images* or *supraliminal images* referring to those of which the subject is conscious and *subliminal images* those necessarily presupposed images that do not at the time enter into consciousness, but whose effect cannot be denied. Cf. Ebbinghaus, 'Grundzüge der Psychologie,' I., p. 53: "Wir gelangen also zu dem Resultat: unbewusste Vorstellungen sind zwar nichts den bewussten und uns bekannten Vorstellungen direkt Ähnliches, aber sie sind trotzdem als etwas Psychisches irgendwelcher Art anzuerkennen. \* \* \*



flashes of light with other empty time or with time filled with the same kind of light flashes. Miss Nelson says that this is what was done with short times by Meumann. But between the researches of Meumann and Miss Nelson there is a great difference, as will presently appear. In the research now under consideration the results were conflicting. It seems inevitable that if the present writer tried that experiment, the empty times, if he 'sat in a darkened room' in absolute quietness, would be subjectively filled with auditory imagery. This auditory imagery might conceivably take the form of a conversation in words or of a piece of music. In either case the original purpose of the experiment is frustrated. It was to compare the estimate of times objectively filled with visual stimuli, with times objectively empty (in some of the experiments), the result being the demonstration of an illusion (which for one of the subjects grew less as the standard time was increased from one half a minute to ten minutes). Now with the present writer, who has made a number of tests on the ability to estimate time intervals by means of auditory imagery, the illusion would vanish entirely after he had trained himself to use a certain piece of music as a measure of the time. Thus, *e. g.*, mentally start the theme of the *allegretto* movement of Beethoven's Seventh Symphony when that light was seen which marked the beginning of the standard time length, and note the bar of music when that light appeared which marked the end of the standard time length; and repeat the process, beginning with the flash marking the beginning of the portion of time which was to be subjectively measured. Whether the subjectively measured time were filled with flashes or not would make little difference if the attention were concentrated upon the internal music. But, it may be answered, the subjects in this experiment were asked not to use any such method. Miss Nelson has not stated whether this is the case or not. If they were, it would still amount to the same thing, for it would be impossible to prove (from what is told us of the conduct of the research) that *subliminal* images had not had some effect upon the time estimate. But Miss Nelson reports that the filling of an interval 'does not affect all three subjects alike.' Now this fact may be explained in the manner which I have just indicated, *i. e.*, a train of auditory imagery which has the objective quality of temporal protensity, may have helped some of her subjects. It may not have. But Miss Nelson has not told us positively that her subjects had no auditory imagery, and until she does, we cannot think that the experiment was performed with due regard to the mental content of the subjects.

The only remarks Miss Nelson makes about the introspection of

her subjects is that they had difficulty in keeping their attention on the length of the standard (p. 449); consequently in some of the experiments she told them whether the standard was to be filled or unfilled, long or short; and that they 'expressed a dissatisfaction with their estimates and felt that they made little, if any, difference between the longer intervals' (p. 454); and on page 455, "the difference due to the filling was, I think, merely a difference in the direction of attention, the monotonous regularity of the lights being, in general, a means of holding the attention and preventing the mind from wandering. From this point of view the filled time was psychologically the more empty or barren of the two \* \* \* the time being filled with monotonous sensations of light, but empty of vivid or interesting trains of thought." There is another remark that indirectly throws some light (p. 454). "During the longer periods it was impossible to keep the attention so closely fixed as during the intervals of one half and one, or at most two minutes. It is at about this point that the change of sign occurs in the estimates. The general feeling of weariness seemed to be the chief criterion in the longer intervals." The present writer thinks it extremely unfortunate that Miss Nelson did not either give her subjects something wherewith to keep their attention directed upon the standard or ask them what their mental content was during the time that they were paying attention to the standard. It seems necessary that the subject have some train of thought during the 30 to 600 seconds that they sat waiting for the flash to come which marked the end of the standard time. It is conceivable that after the first filled time they may have had (during an unfilled time) visual images of the flashes of light. It would be interesting, not to say important, to know this because we might voluntarily image flashes in rhythm. It is also conceivable that the subjects or any given subject may have had auditory imagery. As I have above shown, auditory imagery has a very real temporal quality; so that if the subjects of the present research had auditory imagery during the time of the experiment, it would be at least interesting to know this. It is possible, too, that any other of the half score or so of available kinds of mental imagery may have been in the consciousness of the subject, but we are not informed as to the facts. Now the really important question is the relation these different kinds of images bear to the time sense. Auditory images seem to the present writer to be the best as a means for judging time; but he realizes that motor or tactual might do the same service in other observers. The question as to the estimation of filled or unfilled time really, therefore, involves the question as to the elements

of the stream of consciousness during the unfilled time. Here, however, we have a psychological experiment in which the conditions are all arranged for introspection, and, in spite of the fact so well stated by Professor Titchener, that 'A psychological experiment consists of an introspection or a series of introspections made under standard conditions' ('Experimental Psychology, Students' Manual, Qualitative,' p. xiii), the introspections are not recorded, or if recorded, not published. And it is apparent that the figures given are of little or no value, as that which they ought to enumerate is not even mentioned.

It is to be noted in defense of the position maintained above, that Meumann in his article referred to by Miss Nelson ('Beiträge zur Psychologie des Zeitbewusstseins,' *Phil. Studien*, XII., 127) meets the objection that images may creep into the mind during the times he is studying and so perturb the numerical results, by saying that of course there are really no absolutely empty times; but that there is no use in multiplying words over this fact (which he terms a *Binsen-wahrheit*); and claims that in the short times that he used, viz., up to four or five seconds, one could keep images in a state of complete inhibition (in einem Zustand totaler Hemmung). But even Meumann does not claim immunity from images above five seconds, and Miss Nelson begins at thirty and goes up to six hundred. The present writer thinks, therefore, that not only are these experiments unsatisfactory, but they are not related to those of Meumann; as, if Meumann's statement be accepted, there is really an entirely different factor in the present experiments which has not been taken into account.

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## PSYCHOLOGICAL LITERATURE.

*Einleitung in die Philosophie.* HANS CORNELIUS. Leipzig, B. G. Teubner. 1903.

Professor Cornelius defines philosophy in terms of endeavor, not of content, as 'Streben nach letzter Klarheit.' He examines the dogmatic methods, 'material-monistic' and 'idealistic,' and finds that both have failed of reaching this 'ultimate clearness.' The only adequate method is, he declares (S. 164), the 'empirical,' which starts from facts, not from assumptions. But all facts are, in the last analysis, facts of consciousness, and a study of consciousness is, therefore, a preliminary procedure of philosophy, which is in so far identical with psychology (S. 168).

A large part of this 'Introduction to Philosophy' consists, thus, of psychological analysis and classification. The most significant feature of the analysis is its recognition of the relational elements of consciousness, the *Gestaltqualitäten*: plurality, similarity, identity and the others (S. 208 *seq.*). The author lays his greatest stress, however, on the unity of experience, pointing out (S. 206 *et al.*) that the elements of consciousness are distinguished only by abstraction and that every experience comes to us as term of a series or member of a system (S. 248). "The fundamental fact, \* \* \*" he says, "is no other than the unity of our psychic life, by virtue of which every event must stand in definite relations to others" (S. 250).

From this direction, the writer reaches the conception — closely akin to Kant's — of the objective world as 'the orderliness of our perceptions as known through experience (*die erfahrungsmässig erkannte Gesetzmässigkeit* unserer Wahrnehmungen' (S. 264). The permanence which characterizes the world of experience, is due simply to the constancy of our meanings: "We have found, for example, that there are objects characterized by the attributes which we learn to know under the name of salt. In future, therefore, we can classify an object as salt only if we discover in it all the attributes which characterize this concept in its previous meaning" (S. 291). And the "universal law of causality is nothing other than the demand — indispensable to the unity of our experience — for the ordering of all phenomena under constant empirical connections" (S. 294).

To the writer of this review, the great defect of the book is the

one-sidedness of its idealism. The thoroughly idealistic character of the world of experience and the impossibility of a world beyond consciousness are brilliantly demonstrated; but there is nowhere any consideration of the selves who experience and are conscious. The 'I' is treated simply as one phenomenon among others (§ 31); and the existence of other selves is regarded (S. 323) as a mere probability. The result of this neglect of the self beneath phenomena is, first, a philosophic system, partially the Kantian, which lays stress upon the categories but leaves out of account the 'transcendental I,' and second, a phenomenalist psychology which inadequately describes our consciousness in its fundamentally social aspects.

The book is clearly and well written and abounds in telling illustration. The criticisms of philosophical systems are skilfully introduced and are almost uniformly valuable.

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*The Psychology of Ethics.* DAVID IRONS. William Blackwood and Sons, 1903. Pp. xviii + 172.

Dr. Irons' little book naturally falls into two parts, as he tells us in the preface, the first covering the same ground as, and differing little in statement and not at all in theory from, his articles on the emotions that appeared in the *Philosophical Review* about 1897; the second dealing with man's active nature, and seeking to establish the ideal of worth as its supreme regulative principle, this last position having been in part anticipated in the author's 'Natural Selection in Ethics' that appeared in the same periodical.

The author's main points may be briefly suggested. As an essentially active being, man is characterized by many primary tendencies to reaction; pleasure is the result of their success, pain of their failure; and the emotions are various 'feeling-attitudes' excited by situations made, and felt to be, significant because of their relations to the above reactive tendencies, and to the interests incident to them. And pleasure, pain, and emotions, of course, add secondary tendencies to reaction to the primary tendencies mentioned.

Were this the whole of man's active nature, chaos would result. But he is also characterized by a tendency to realize his ideal of worth, his notion of what he owes to himself, and 'all his particular impulses must be brought into the service of this end.' Moreover, this end is eminently concrete, what each man owes to himself being relative at once to his nature and capacities, and to his place in the social organism.



One lays down the book with some reservations incident to Professor Irons' deliberate dependence on unaided introspection. Are not the theories advanced colored by the exclusive observation of a nature reflective and philosophic out of the common, and do they not, in resting on the observation of an individual, unduly neglect the social bearings of man's moral nature?

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*De la Réalité du Monde Sensible.* JEAN JAURÈS. Deuxième Édition. Paris, Alcan. 1902.

In this volume of 428 pages M. Jaurès presents an elaborate system of philosophy in which the chief problems of metaphysics, cosmology and psychology are about equally considered. It is manifestly impossible in the space at our disposal to do even rough justice to a work of such scope, and we shall content ourselves with a brief summary of the author's main conclusions, and a few comments upon the points that would seem of most interest to the readers of this journal.

M. Jaurès is at once too eclectic and too original to allow of our classing him with any one school. The cardinal principles that appear to govern his philosophy are: First, the Rosminian acceptance of *Being*, in its two aspects of activity and potentiality, as the highest and most significant category; second, the Cartesian identification of all energy with motion, and the consequent correlation of all sense-forms with modes of motion; third, the Spinozistic belief in extension as a true and essential attribute of the one Reality; fourth, the Hegelian and Schellingian belief in the purposeful, meaningful, and living character of physical nature.

In his first chapter the author considers the several criteria that are actually used to distinguish the real from the unreal. Persistence in time is the first mark of reality. That which is felt as the same at different moments is regarded as real. Touch gives a more vivid notion of persistence than sight, hence the tactual and with it the resistant are accepted as marks of reality. The coincident testimony of sight and touch is, however, a truer criterion than either alone; indeed it is not until we get the various attributes simultaneously presented through different senses that we clearly perceive the real. But many attributes cannot be thought of as existing together without some inward bond. The true reality for perception is thus a *substance* with its attributes. Now the unity of a substance could not be apprehended in perception unless the intellect possessed *a priori* the con-

cept of unity. It is then the intellect which enables us to perceive reality; and the *de jure* criterion to which the examination of the *de facto* criteria has led us is *intelligibility*. The real is simply the intelligible. Dreams are rejected as unreal because though containing a measure of connectedness they lack the fuller unity and intelligibility of waking life.

Having thus attained a criterion for distinguishing the real from the unreal, our author is ready for the task which gives the title to his book. He will prove 'the reality of the sensible world' by showing that world to be intelligible, first in its general constitution, second in its specific manifestations.

It is a notable conception, this of M. Jaurès. As a natural realist and in the interest of natural realism he proposes to take as supreme criterion a principle which has for the most part been regarded both by realists and non-realists as the exclusive property of idealism. The world is real, but it owes its reality to its rationality. While we sympathize profoundly with our author's contention that a rationalistic realism is the only realism that is capable of withstanding the onslaughts of idealism, we must dissent from his interpretation of intelligibility as being far too narrow. Dreams and illusions usually possess a measure of intrinsic disconnectedness and irrationality, but M. Jaurès appears to us quite wrong when he asserts this irrationality as the ground for their unreality. Such disconnectedness is in fact often lacking in the dream, while the quasi-dreams of art are entirely without it. The world in which Hamlet moves has surely a finer intelligibility than the humdrum world of our waking experience. We reluctantly reject it as unreal, not from any want of intrinsic intelligibility or possibility, but because it lacks extrinsic intelligibility or 'compossibility'; it cannot be harmonized or made continuous with the totality of our waking experience. It is thus extrinsic intelligibility or compossibility which together with intrinsic reasonableness constitutes our criterion for testing reality. And it is only by considering this second aspect of intelligibility that we can appreciate the justness of the empiricist's contention that that which is *given* through the senses and independently of the will is the primary reality with which all other experience must accord on pain of being classed as 'unreal.'

The first use which the author makes of his criterion is to deduce from it *a priori* the general constitution of reality. The intelligible must be first of all *one*, but it could not be merely one without, so to speak, collapsing into nothing. It must therefore manifest itself in a system of many intelligible unities. These must be united by the ex-

ternal bond of spatio-temporal causality and also by a common striving toward the ideal. The ideal, however, could only be attained by beings that existed continuously in space and in time, and possessed a nature partly actual and partly potential. Indeed Being, which lies at the heart of everything, may be seen to have two aspects, actuality form or quality, and potentiality extension or quantity. Being *in actu* is God, Being *in potentia* is the World. These two aspects of Being are co-eternal though potentiality is logically second to actuality and ontologically dependent on it. The manifestations, or, as Spinoza would say, the 'modes' of Being result from the eternal permeation of the 'attribute' of potentiality by the 'attribute' of actuality. They thus reproduce in their own nature the duality of Being. Looked at from the side of quantity or potentiality they are motions, while from the side of form they are sensations.

So much of the general character of the real world can be seen to follow *a priori* from the demand for its intelligibility. But these high and general deductions need to be inductively verified and extended by a study of the manifestations of Being in their detail, and the six remaining chapters treat in turn 'Motion,' 'Sensation and Quantity,' 'Sensation and Form,' 'Space,' 'The Infinite,' and 'Consciousness and Reality.'

In the chapter on motion, it is shown, first, that motion itself reproduces the eternal duality of being *in actu* and being *in potentia*. Motion even as such is not purely indeterminate or quantitative. It must have a definite form or character. Modern science regards 'gross matter' or that which moves as being itself a mode of motion. The real substrate of motion is ether, or simply, extended homogeneous being. The unity of this being is manifested in the indestructibility of matter and energy.

The question as to whether matter originates *in time* from the ether is considered and answered in the negative (p. 127). Each distinct form of motion is correlated with a form of sensation, but each form of sensation has a unique and irreducible meaning, and hence cannot have had an origin. In pure ether only undifferentiated motion would be possible, therefore differentiated material bodies share the eternity of the sensory forms with which they are correlated.

The most important point in the chapter on motion is the defence of the Cartesian view that all energy is energy of motion. Force or potential energy is simply 'motion that completes itself in infinitesimal time' (p. 75). Instead of developing and elucidating this promising though somewhat enigmatic definition, the author, as it seems to us,

weakens his case by attempting to show that the truly kinetic character of 'potential' energy is evidenced in the molecular changes that result from the *fatigue* of bodies subjected to strain.

In the chapter on 'Sensation and Quantity' many considerations are adduced to show that sensations are not only related by quantitative laws, but that they actually contain quantity. Selecting M. Bergson as representing his opponents, our author argues (pp. 164-168) that the contention that sensations differ only in quality is false for two reasons. First, the objective quantities of the physical world could only be presented in perceptions which participated in their nature. The soul could not perceive motion and extent unless it moved and was extended. Apparently M. Jaurès would not shrink at all from the paradox that the perception of a square is a square perception. This revival of the quaint psychology of Democritus will be less convincing to the average reader than the second reason which is alleged in support of quantified mental states. We estimate the quantity of an object by the quantity of sensation that it produces, and not *vice versa*. If two balls, one large and hollow, the other small and solid, be dropped to the ground, the greater intensity of the sound coming from the smaller is regarded as an immediate and sufficient justification for attributing to it a greater mass. The fact that each change in the intensity of a mental state involves a change in its quality is the only objection which M. Jaurès considers as at all serious. He explains it, however, as the result of the limited capacity of finite consciousness. On account of this limitation every quantitative change alters the structure or quality of the psychosis in which it occurs.

The theme of the chapter on 'Sensation and Form,' is the extra-mental reality of the objects of the several senses. They are shown (1) to require an extended world for their manifestation, and (2) to be expressions of eternal meanings. These 'secondary' qualities constitute a bond between the 'primary' qualities of being *in potentia* and the ideas of pure intellect — being *in actu*. Thus light expresses the unity of being with itself, and is consequently correlated with motions in the one homogeneous ether. Touch, on the other hand, expresses the irreducible disparateness and individuality of the many material manifestations of being. Sound expresses the communication of beings with one another.

The chapter on 'Space' is chiefly interesting for the criticism of Kant (pp. 335-340). Space is admitted by Kant to be the 'image' of quantity. But quantity is a category, and as such possesses, if not an objective, at least a transhuman validity, hence space as its 'image'

should possess an equal validity, and not merely the arbitrary and human scope which Kant ascribes to it. Moreover, unless the forms of sensibility were intrinsically connected with the categories, through the idea of being (which Kant overlooks), it would be impossible for the 'imagination' to effect their union.

In the chapter on the 'Infinite,' the author devotes himself mainly to vindicating space from the charges of self-contradictoriness that have been made against it by Zeno and Kant. Those philosophers err in regarding space as a composite or aggregate, when, as a fact, it is a continuum and therefore a unity (pp. 350-360). The parts which we think of as composing space are nothing but arbitrary and subjective divisions. Space as a whole is logically prior to all such 'parts.'

The last chapter discusses the relation of consciousness to reality. M. Jaurès admits to the idealist that consciousness or egoity is as truly an aspect of being as is extent. Every finite ego presupposes, and, in a sense, contains the absolute ego. We must not, however, accept solipsism. We can only reduce the universe to our ego by raising our ego to the infinity of the universe. The Absolute or God is a conscious ego, but as he is possessed of no organism he cannot be regarded as an individual or as a person (p. 427). Even in the sphere of finitude we can feel that individuality is not essential to consciousness. The alleged facts of telepathy show the possibility of extending our consciousness far beyond the limits of our organism.

M. Jaurès' work seems to us, on the whole, an important contribution to philosophy. As literature it is wholly charming. The book is pervaded by an atmosphere of poetry and mysticism that harmonizes curiously well with the author's Cartesian lucidity of style, and his love for quantitative conceptions. And here, as in the work of Guyau, the illustrations which are used, and the piquancy with which they are presented, go far to reconcile us with details that from a coldly philosophic standpoint might appear as fantastic and inconsequent.

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*Psychologie du Rire.* L. DUGAS. Paris, Alcan, 1903.

Another of the French monographs, written under the fruitful influence of Ribot, this study of laughter takes as its text one of the theses of the master in his 'Psychologie des sentiments.' "Laughter," he had said, "manifests itself under so many and such different conditions—physical sensations, joy, contrast, surprise, the bizarre, the strange,



the base—that the reduction of all these causes to a single principle remains quite problematical.” Following out this thesis, M. Dugas comes to a wholly negative conclusion as to the ability of scientific method to give anything like a unitary explanation of the many phenomena of laughter. The work is thus wholly descriptive and critical, contains no new principle or hypothesis, unless indeed this negative attitude be looked upon as a novelty. His first point of critical attack is the conception that all forms of laughter can be reduced to a unitary physiological explanation. The physiological theories of Spencer and Bain which will explain elementary and simple forms of laughter, are, however, too abstract and simple to comprehend the species of laughter described as ideo-emotional. For these phenomena other principles, intellectual and moral, have been introduced by thinkers of varying tendencies. The intellectualistic theory which would reduce all forms of ideo-emotional laughter to reaction upon contrasts and contradictions of ideas; the pessimistic theory which would find the source of such laughter in the ethical moment, the sense of superiority of self over others, are in turn subjected to a criticism, the outcome of which is the recognition of these as subsidiary principles of explanation, as applicable to limited groups of facts, but which have, unfortunately, received an undue extension at the hands of men of genius with whom they had become fixed ideas.

His own view leans more toward what he calls, in distinction from the preceding theories, the æsthetic. All humor, ideo-emotional laughter, begins as play. “But from the fact that we recognize play as the essential and characteristic element in all laughter it does not follow that all forms of laughter are thereby reduced to unity. On the contrary, by its variations, playful laughter engenders the different species of humor.” Laughter, which began as a playful impulse, differentiates itself into the intellectualistic and quasi-moral types. The latter are, in a sense, degenerations of primitive laughter.

M. Dugas has essayed a qualitative and quantitative classification of ideo-emotional laughter, according to the character of the emotions expressed and the emotional intensity of the expression. The quantitative classification is based upon linguistic symbols for different degrees of laughter but, since only the French language is taken into account and no attempt at comparative study is made, the result is suggestive rather than convincing.

In addition to this negative scientific view of laughter, he has also developed negative conclusions as to the moral and social functions of laughter. Laughter is in itself not a selective principle in ethical

judgment, stigmatizing that which is opposed to the ideal of human perfection or that which varies from recognized social constants of judgment, sentiment and character. It is, in itself, non-moral and non-social and only accidentally produces effects of moral and social value. Ridicule may equally produce immoral and anti-social effects. His conclusion then is that laughter, from every point of view, is an accident, an expression of the individuality; it discloses as many forms as there are different characters, minds and states of the mind. It seems then to the author that it cannot in any true sense become an object of science. Practically, it may be an object of desire or aversion but not of volition — it cannot be taken for an end of action.

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*La Mimique.* ÉDOUARD CUYER. Bibliothèque internationale de psychologie expérimentale. Paris, Doin, 1903.

This monograph by a painter and professor of anatomy has only an indirect value for the psychologist, for while it undertakes a very minute study of the variations of emotional expression in the different parts of the body, it is the expression for the artist which is constantly kept in mind and there is very little, if any, analysis and classification of emotional states on distinctively psychological principles. If, however, the student of emotional expression should ever develop a method for the experimental isolation of qualitative differences in emotional states, he would have a wealth of fact at his disposal as a result of M. Cuyer's painstaking work.

The author's method of determining the facts of mimetic expression is both analytical and synthetic. On the one hand the various muscles and groups of muscles of the face, head, arms, shoulders and lower limbs, etc., are all examined separately to determine, in connection with the separate emotions, the slightest variations of movement which will suggest distinct emotions. This analysis of expressive movements is controlled by the principle that the breaking up of emotional expression shall be carried only so far as a change in a single feature will unmistakably signify a distinct emotion, as when changes in the angles of inclination of eyebrows or lips have these suggestions. The minuteness with which these variations of eyebrows, nose, mouth, lips, wrinkles on the forehead, movements of the ears, accompanying variations of emotions, have been quantitatively studied with the help of photographs and works of art (there are 75 figures in the text) is extraordinary.

The synthetic study, on the other hand, seeks to reconstruct the various emotions in their fulness by combining the various movements thus analyzed out into total expressions. The author discloses no laws governing these combinations. In fact, his method is wholly empirical. He gives us some 115 varieties of emotion in alphabetical order with their corresponding synthetic expression. It is certain, he tells us, that the number of combinations is practically infinite as the variety of nuances of emotion is indeterminable. The only general principle which he notes as governing these syntheses of elementary movements and attitudes is the distinction between normal and abnormal combinations. Following Professor Pierret, he finds the synthesis of expressive movements in the normal individual rapid, concordant, adequate, homogeneous and persistent. In the abnormal individual they are slow, discordant, excessive or insufficient, disassociated and fugitive.

Some of the psychological assumptions upon which M. Cuyer has based these analyses and syntheses are open to criticism. This is notably the case when he supposes that involuntary and voluntary, or mimetic, expression of emotion may be taken without distinction as the basis for his studies. In mimetic expression, however, there are certain processes of abstraction and conventionalizing of movements the psychology of which the writer has ignored entirely. With all these psychological defects, and they are in some ways serious for the psychologist, although perhaps unimportant for the scientifically minded artist for whom the work is primarily intended, it remains true that this study contains material of value and is, in a way, an extension of the work of Darwin, Bell and Duchègne. A historical résumé of the contributions of these writers to the subject adds to the worth of the monograph.

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*On Active Attention.* F. H. BRADLEY. *Mind*, XI., No. 41, Jan., 1902, pp. 1-30.

The purpose of this article is 'to fix the meaning of active attention in accordance with the ordinary usage of language, and next to deal with a certain number of questions concerning it.' The word attention is to be used in the sense of active attending, and the reader is asked not to forget that volition is assumed to consist in 'the self-realization of an idea.'

"The mere having of an object or objects is by itself not attention."  
 "To attend in the proper sense I must by my action support and main-

tain an object in myself, but we have attention only so far as I maintain it theoretically or at least perceptively." Moving one's hand or eye to gain knowledge about an object need not involve attention. "My end in attention is to maintain an object before me with a view to gain knowledge about it." Attention is thus negative of any mere psychological interference with the object and its knowledge.

But attention implies also a volition on my part. When listless or absorbed, I may notice a bird fly across the field of vision without attention. An idea may develop itself theoretically before me without attention. Not that attention is the same thing as will, and not that all attention is directly willed. It may be directly willed but need not be so. Wherever an end of any kind involves in and for its realization the maintenance and support of an ideal object before me and in me — that is attention (p. 8). "The ideal development of the object in me is thus, directly or indirectly, the realization of my will" (p. 10).

So-called passive attention 'may be called the mere occupancy of myself,' and this is not essential to attention. Immediate action upon a sensation or a perception need not involve attention; and apperception, the modification of a sensation by a disposition, is not an attending. And yet, this activity of apperception 'may be said, if you please, to cause in a certain sense attention to the object' (p. 11), but we have first been impressed and laid hold of by an idea (=any suggestion even when coming straight from a perception) (p. 29). "Our will to realize this idea in external action and in inward knowledge is but the self-realization of the idea which so has possessed us. And you cannot, if you keep to facts, maintain even that the suggestion holds us in all cases because it arouses desire or even pleasure." We cannot get rid of ideo-motor action, and it is idle to deny that at least some idea-motor actions are volitions. Sometimes an idea whose psychological origin is apparently casual or undiscoverable is simply 'there' and remains 'there'; it 'goes on to realize itself and in this way unfeeling forces, we may say, our will and our active attention' (p. 30).

*On Mental Conflict and Imputation.* F. H. BRADLEY, *Mind*, XI., No. 43, July, 1902, pp. 289-315.

Divided will, conflict of ideas in desire and impulse, alleged action contrary to will, and the principles on which we impute actions to ourselves, or again disown them, are the topics of this paper. "Volition I take to be the realization of itself by an idea, an idea (it is better to add) with which the self here and now is identified" (p. 290).

The paper first discusses the alleged case of action realizing an idea contrary to will — such as yielding to morbid desire for drink. The author holds that two ideas cannot be present at once, and that where they immediately succeed the one the other the self is not identified with both of them in the same degree. All my acts are mine, but they are not equally or in the same sense mine, as is shown by the following: (1) Viewing the self in its material aspect, we all distinguish between our true self, our self taken as a whole, and the lower or chance self of any moment. (2) Formally, we regard the more universal as the higher and the more mine; but the universal is often abstract, and hence may be higher in one respect while in other respects it is lower and worse. (a) A course will be formally higher when it explicitly and consciously asserts a principle, instead of embodying it unconsciously. (b) To adopt a course reflectively is higher than to adopt it at once and unreflectively. (c) *A* and *B* may be incompatible and known to be so, each involving the negation of the other, or they may be so related that *A*, for example, includes *B*, and in the latter case *A* will be the higher and the more mine. (3) An idea which is pleasant or more pleasant is so far higher and more mine, and one which is painful or more painful is lower and less mine.

The author next proceeds to give reasons for not accepting the alleged fact of action contrary to will. In the end he holds that if the alternatives are really incompatible and are known to be so, they cannot, 'while really taken thus as alternatives, be present together, and we are able to think this possible only because we really do not take them as opposites.' A man cannot knowingly and willingly do what is bad. And yet, the author admits that an idea which is contrary to will may get itself carried out (where the ideas are abnormal), denying merely that any act of this sort is volition. The reader feels at first that Mr. Bradley has yielded his case, at this point, to Mr. Shand, but he goes on to say that the idea which is carried out in such a case is really not an idea at all, because held subordinate to the alternative idea and negated by it (p. 309). Mr. Shand contends that ideas may realize themselves, in Mr. Bradley's sense of the word, without volition taking place.

If I hold the idea of another person's doing a thing and this thing follows in me, this is not volition; and if I imagine myself in a certain state, and my imagination is thereupon realized, this is not volition; for in neither case can the result be shown to follow as a genuine consequence of the idea, and in neither case is the idea had in mind the one realized in action.



The paper closes (pp. 314-315) with a restatement of the principles upon which the difference and the degrees of mine and not-mine rest. (1) If I can bring and retain *A* not-*b* before my mind and cannot do this with *B* not-*a*, *B* is so far higher and so far mine more truly than *A*. (2) The same is true, if, taken on the whole, *B* is more pleasant or less painful than *A*. (3) If *A* is the outcome of and represents deliberate choice, while this is wanting in the case of *B*, *B* is so far the lower and less mine. (4) If *A* appears as falling under a principle, while *B* falls under a principle lower and less general or under no principle whatever, *A* will to that extent be higher. (5) Lastly, the most important criterion of all consists in the material difference of content. If *A* represents some minor interest of my being, and if this feature is not contained or is to a less extent contained in *B*, *B* is so far lower and is not mine.

*The Definition of Will.* F. H. BRADLEY. *Mind*, XI., No. 44, Oct., 1902, pp. 437-469.

This paper proposes an explanation and defense of the author's definition of will as 'the self-realization of an idea with which the self is identified.' Volition involves the following aspects: (1) Existence, (2) the idea of a change, (3) the actual change of the existence by the idea to (4) the idea's content and in such a way that (5) the self feels itself realized. Assuming 'provisionally the existence of what is called ideo-motor action,' defined as 'the tendency of an idea to realize itself,' the paper proceeds to discuss the above five points.

(1) Existence is either the actual series of events that is now and here, or continuous with my now and here. (2) This existence must be altered in volition beginning 'now' with the 'present.' Even the will to continue the present in a certain character is a will for alteration. The alteration must not be merely ideal, but it must be an alteration to the character possessed by the idea, and it must be produced by the idea. Here follow discussions of several phenomena which in the author's view are not volitions, viz., resolve and intention, will in paralysis, and disapprobation or approval. In resolve, the existence to be altered by the idea is severed by a gap from the actual present; and after abstracting from the result, that is, from the actual realization of the idea, in volition, what remains is so far only an incomplete will (p. 446). We may roughly distinguish between two stages in volition, (1) the mere prevalence of the idea, and (2) the advance of the idea beyond its own existence toward its physical or psychical end. If taken strictly, the first stage 'is not a complete or

really an incomplete act of will,' and yet, 'viewed otherwise and under some conditions, the prevalence of the idea does amount to an incomplete but actual volition' (p. 446). The prevalence of an idea is a process whose stages the author goes on to develop, and the last step in the process in most cases involves an actual change of fact to correspond to the idea. "There will not be any case even an incomplete volition unless to some extent the idea carries itself out beyond itself. Where this aspect fails, there will at most be a doubtful experience, due to a confusion between imagination and fact." Even in the paralytic's will, this progress of the idea beyond itself is to some extent present, and the will to recollect also illustrates the point. As to approval and disapproval, "To approve of things as they really are or as they are imagined to exist, is to take an attitude in itself contrary to actual will. \* \* \* Disapproval in itself is not will and, so far as it becomes will, it falls under negative volition" (p. 454).

The author next discusses the doctrine that all will involves a judgment or belief about the future or at least about the possibility of the end, coming to the conclusion at last that such judgment is not essential to all volition because it cannot be discovered in all volition. Two circumstances combine to make this doctrine seem plausible: (i) we often express the fact of a volition or a resolve in a judgment, and (ii) one cannot will to realize the impossible, and hence it would seem that in willing we must judge the end to be possible. Neither is desire essential to will, nor is choice, nor is active attending.

The discussion proceeds to the objection that an idea is not essential to will. This objection is based on what the author regards as a mistaken view of the nature of ideas, and an account of ideas is here introduced. (i) Our apparent idea and our real idea may be fundamentally different—the former may be but a part of the latter. (ii) An idea may exist in volition and may yet be unspecified and general. (iii) An idea is itself not an image, nor is it always even based on an image as distinct from a perception. An idea may be the 'meaning' of an object, a meaning which does not involve an image and which may be detached from the object. And yet, in all this, the reader is apt to feel that ideas are not defined, and that much vagueness still hangs about them. Actions from imitation, from word of command, and from impulse illustrate volitions which are said to be without ideas, the action being suggested by some perception. Here the author holds that 'If in the act an idea is suggested and realizes itself, that act is volition, unless the idea has in some way lost its own character and has in effect carried out something which is not itself.' For

example, "The idea of another man striking, if as such it causes me to strike, is so far not a volition. And the same conclusion holds if the idea was of another desiring or ordering me to strike" (p. 466). Finally, the author briefly discusses the mistaken view that the end in volition must be realized for us, and that it is so realized when our idea passes into perception. One may desire objects which he knows, or might know, will never satisfy him. And yet, 'Desire is an inconsistent state, I agree, and its inherent contradiction, I agree, should be removed by satisfaction' (p. 468).

Of these three weighty articles, one fifth of the lines are in footnotes, and the author's method is that of defending and proving propositions which have previously been laid down in other writings. Doubtless many readers have found the task of mastering them somewhat difficult, and at the risk of exposing his lack of penetration the present reviewer is disposed to mention some of the sources of his own difficulties. 1. In the paper on 'Active Attention' (*Mind*, N. S., No. 41), we are told that active attention is not necessarily directly willed (p. 8), and is then referred to as 'the willed procurement' of psychical occupancy and dominance. Were this merely an accident in the use of words, it would not need mention here. Understanding that active attention is not always directly willed, what is the difference between active and 'passive' attention? Passive attention, attention 'in the low and perhaps improper sense of psychical dominance and occupancy,' is referred to on page 29 as a use of the word in an 'improper sense.' One of the subtle themes of the paper seems to be that all attention, properly speaking, is active. The reader is repeatedly warned in footnotes and elsewhere that the word attention is always used in the sense of active attention except where other words are explicitly used, and yet one feels that the word attention (unqualified) is not consistently used in the sense of what is *usually* called active attention. "We may will and may attend actively because we have first been compelled to 'attend' passively. \* \* \* We may end in such cases, and we probably do end, by attending actively to the idea, but we may do this because and only because the idea has laid hold of us passively. Our will to realize this idea in external action and in inward knowledge is but the self-realization of the idea which so has possessed us." An idea sometimes 'unfeelingly forces, we may say, our will and our active attention' (p. 30). Just where the compulsion to 'attend' ceases, and 'our will to realize this idea' begins, is not clear from the author's language.

2. In speaking of conation, attention involves an opposition between existence and idea, but this opposition does not entail effort of any kind; it may cost little more than to anticipate its removal ideally (p. 28). But such opposition is, as such, unpleasant, and the tendency of all such oppositions to pass beyond themselves and become something different is usually consciously present. Whether this constitutes conation or not, surely the relation of pleasure-pain to attention is involved in this discussion.

3. The uses to which the term idea is put raise questions which will be referred to below.

4. The second paper is a discussion of action realizing an idea contrary to will, suggested by articles of Mr. Shand. The latter holds that an idea may realize itself in action and still not be voluntary, as it perhaps should be according to Mr. Bradley. The case of yielding, contrary to will, to a morbid desire for drink, is mentioned. Mr. Bradley says that two ideas cannot be present at once, and that when they alternate, the self is not identified to the same degree with both of them: they are not genuine alternative ideas, in other words. The man drinks contrary to his will, but the drinking is not willed. Very true, but an idea is apparently here realized. No, replies Mr. Bradley, it is not an idea, properly speaking, which is here realized. What then, we asked ourselves, is an idea? To this question we find no complete answer in these papers. On the other hand the case of the drunkard seems to answer to the description on pages 29 and 30 of *Mind*, N. S., No. 41, as a case where an idea is simply 'there,' remains 'there,' and goes on to realize itself by 'unfeelingly' forcing our will and our active attention: it seems like the 'self-realization of the idea which has so possessed us.'

5. The account of the criteria of 'mine' and 'not-mine' (No. 43, pp. 314, 315) represents the evaluating consciousness as less organized than it seems to be in experience. If volition were the self-realization of an idea, simply that and nothing more, the criteria of value, or of 'mine,' ought to be purely logical, as in 1, 3 and 4 above. The author has however added (recently, I suppose) to this definition of will the phrase, 'with which the self is here and now identified'; and this gives the fifth criterion above. Finally the pleasure-pain consciousness is, without being correlated with either the self or the ideas which realize themselves, represented in the second criterion. Possibly three theories of the will, namely, as the self-realization of ideas, as the self-realization of the ego, and as the self-realization of pleasure, are here demanding correlation or suprem-

acy. The author's discussion seems to the present reviewer to prove that the definition of will simply as the self-realization of an idea will not hold unless by idea we understand much more than is usually covered by the term.

I briefly note the following questions which were not answered for me, after being suggested, by the reading of the paper on 'The Definition of Will.'

6. What is here meant by *ideo-motor action*? The paper seemed to use the term to cover all tendency of ideas to produce motor changes, and yet only the tendency of 'an idea of movement's sensible effects' to do so is usually meant. The difference between *ideo-motor action* in general and *volition* in particular is not clearly made out in these papers.

7. The author discusses ideas on page 461 of the paper on 'Will,' but his predication are either very formal and general or negative in quality. Whether the idea involved in *volition* is an idea of a remote end, or of a movement's sensible effects, or of a possibility, or of present existence as distinct from the future, or of self, or of reality in general, is not stated. We are told that an idea that is subjected to another idea is not, properly speaking, an idea at all (No. 43, p. 309). When the idea of another person as performing a certain action or being in a certain condition is followed by the same action or condition in myself, and when the idea of myself as being in a certain condition is followed by that condition, 'that which has been carried out in act is no more than a partial aspect of my idea, and it, therefore, in the proper sense, is no idea at all' (No. 43, p. 311). An idea, again, is not necessarily an image. The 'meaning' of a perceived object may be 'detached or loosened' from the object, 'but this loosening does not imply always the existence of an image or images, separated from the object and maintaining themselves' (No. 44, p. 461). In a footnote to page 29, No. 41, 'Idea includes any suggestion, even when coming straight from a perception.' In the paper on 'Mental Conflict,' page 311, Mr. Bradley denies that imitation and suggestion are cases of *volition*, because what is carried out in action is only a partial aspect of an idea, and, therefore, not an idea at all; but in the article on 'Active Attention,' page 29, where he is deliberately characterizing *volition* as *ideo-motor action*, imitative and suggested actions seem to me to come within the scope of his characterization. Only, in the latter article, these processes illustrate the case of an idea laying hold of us and getting itself realized willy-nilly, illustrate, that is, the self-realization of ideas and of *volition*;



while in the former article on 'The Definition of Will,' the idea realized in these processes of imitation and suggestion are not ideas, properly speaking, and cannot, therefore, be regarded as volitions.

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*Choice and Nature.* EDGAR A. SINGER, Jun. *Mind*, XI., No. 41, Jan., 1902, pp. 72-91.

My micrometer readings and my liking for port wine and Beethoven sonatas are alike in inaccessibility to you. Are there any judgments in which the subject is the sole arbiter of the truth of his own statements, so that he can say what he will without risk of error? The sophists said that judgments of 'immediate certainty' are of this nature, but the immediate, if certain, is also idiosyncratic. It can neither be contradicted nor confirmed. Moreover, the assumption that the case can never occur again makes it quite indifferent what judgment is passed upon it.  $+a = -a$  only when  $a = 0$ ; 'the only absolutely free judgment is the meaningless one.' Truth means a mental grasp of reality capable of confirmation or refutation from an indefinite series of other points of view; and the average of these observations is the only 'fact' of either æsthetics or science, although the variable error is larger in the former than in the latter. Consequently, if empiricism "urges that the answer to every meaningful question must be wrung from experience, and hence must involve a question of fact, I think history forces us to accept the dictum. So that if any class of judgments involves the exercise of a choice, it is because the statement of fact itself depends upon choice." But we are not justified in saying that scientific method excludes all choice on the part of the describer of nature.

"Is there only one, or are there more than one, way in which the scientist may present nature as a uniquely determinate process?" If more than one, is the scientific describer's selection capricious or can we discover a principle by which it must be guided if his description is to be true, the nature it portrays real? A zero probable error is, in finite experience, impossible; hence a probable error and the infinite series of points of view which it summarizes are included in the scientist's meaning when he speaks of a fact. All the disjunctions of ignorance and the 'neglects' practiced by science could be included in the form of this 'probable error.' Some philosophers (James) hold that the psychological factors determining the choice between the disjunctions of science have something to do with 'the result';

while others maintain that science always abstracts from nature (Ward) and gives us only an 'ideal construction' on which it would be unsafe to base our view of the world. The disjunction of ignorance is, however, no ground for the play of choice, but only for the wavering of doubt. From this it follows that the very simplest statement that science can make about nature must take on a hypothetical form. And yet, the account of nature which interests us must be expressed in categorical judgments, and science makes categorical predictions. What has become of the conditional clauses? Science absorbs the conditions in the categorical statement by the simple device of 'standard conditions.'

These conventional 'standard conditions' are arbitrary, are social, aim at representing nature as a thoroughly determinate process, and are of such a nature that no categorical account of nature can be given which does not involve a series of such choices. Nature is indifferent to any particular classification, but not to all classification; for choice is involved in anything we do, or can, mean by nature.

The author illustrates and confirms his thesis by referring to the history of science, to the choices which have been made there, and to the way in which they have made 'nature.' These choices have not been capricious, but according to a principle, and this ground of preference is also the ground of truth. These choices are the only *a priori* factors in experience, and this completes the motives for the doctrine of synthetic judgments *a priori*. Choice is illustrated in the distinction between 'artificial' and 'natural' classifications, in the older conception of 'true orders,' and in the language of such scientists as Linnæus who imply that there are classes *in re*. The genetic classification now reigning in the realms of biology is a choice. And classifications always determine the next questions for science. The next question for biology, moreover, concerns other sciences than biology, involving as it does 'mechanical factors' of evolution which are fundamental in all natural science. It is just such insights into the factors involved in any new classification which tell us what nature *is*. Only in this sense can a classification be 'true to nature,' and only in this sense can there be classes *in* nature. Copernicus' system suggested the question of Kepler, Newton, Huygens, Kant and Laplace, whether motion can be subsumed under growth; and at last nature has come to cover an evolution of mechanical processes.

But facts might lead us to reject any of these laws — the *a posteriori* element in knowledge! In other words, we choose that our descriptions of nature shall be simple. We assume that universal

judgments are possible. But why and how are universal judgments possible? Because we have the remaking of facts within our power. In the resistance of facts to our formulæ we simply experience the discrepancy between old choices and present needs; and hence, the search for universal formulæ is bound to succeed. Every one says that the law of gravitation does not express the facts: we have other 'laws' dealing with the exceptions. In other words, we have introduced new classifications which have the conditional flavor of all classifications. Exceptions only invite, they do not force, a rejection of the old classification.

When we wonder at Nature's order and simplicity, it is at our own handiwork. When we find the scheme of things 'sorry' we 'shatter it to bits and remold it nearer to the heart's desire'—only the heart's desire must not be unprincipled. It must abide by the principle of maximum simplicity, economy or unity. Why is this regarded as true? It is in fact a strong intellectual need. Second, it is the principle expressing a universal will. But why listen to intellectual rather than to æsthetic or spiritual needs? The social will is no mere *consensus gentium*: the will we seek must be found *sub specie æternitatis*. We do not mean to stop with any empirical generalization, for our problem is the same as Kant's in the deduction of the 'categories.' Post-Kantian thought has passed from Kant's static to a dynamic attitude toward experience. Not the unity of thought but our thought's struggle after maximum unity constitutes experience what it is. Observations stimulate new interpretations determined by the principle of choice—not the individual's choice, but society's—not that of any particular society, but of all society, to contradict whose will is to destroy the meaning of experience. The question, are not the demands for the goodness and beauty of our world involved in this struggle, is put but not discussed at length.

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*Ueber die allgemeinen Beziehungen zwischen Gehirn und Seelenleben.* TH. ZIEHEN. Leipzig, Barth, 1902. Pp. 66. Mk. 1.80.

This is the German edition of an address which first appeared in a Holland magazine (Gids). The writer's reason for reproducing it is his conviction 'that in the many addresses by great physicians and naturalists on the same subject, the historical and epistemological standpoint has everywhere been kept too much in the background.'

A brief history of this problem in ancient and medieval thought,

interesting for its mention of many unfamiliar names, leads up to Cartesianism and the Associationists. Many attempts were made by the anatomists and physiologists of the seventeenth and eighteenth centuries to find the organ of the soul in the brain. At the beginning of the nineteenth century, Gall and Spurzheim emphasized the view that the soul life is directly related to the brain, and Flourens, by experiments on the brains of living animals, showed that all that is characteristic of spirit disappears with the disappearance of the cerebrum. Foville and Delaye studied pathological cases and began dissection of the brain. Then came the localization theory, finally established by Broca in 1861. The author gives a brief review of the pathological and other evidence for the localization of brain-functions, leading up to the doctrine of psychophysical parallelism. After explaining this principle, the question is asked, 'What relation obtains between material processes and our sensations?'

First, a group of insincere answers to this question: the answer of those who deny the facts of localization and parallelism, that of those who ignore science entirely, and that of those philosophers who, beginning with the absolute, still hope by a play of logic to deduce a world and more. The sincere theories are either dualistic or monistic. Dualism breaks over the question, 'How comes it that out of all the numberless material processes, only those of the cortex are accompanied by psychic processes?' The psychic is known only as the conscious, and those who look upon it as an epiphenomenon, see it as forever mysterious and unintelligible. Another form of dualism goes by the name of psychophysical causation, holding that through the body, material things act on the soul and the soul on material things. This view is opposed to the scientific principles of the conservation and persistence of motion and the conservation of energy. Some have maintained that when the material world acts on the soul, kinetic energy is changed into potential, and vice versa when the soul acts on the material world. But in this case we have monism of the materialistic type, and not dualism.

In his discussion of monistic theories, the author first mentions what he calls pseudo-monism, the doctrine that the mental and the physical are two attributes, or manifestations, or aspects of one reality. Here belong Spinoza, Fichte, Schelling, Spencer, Münsterberg, and others. Similarly, Fechner, the physiological psychology of Ebbinghaus, and others, hold that the two are the inside and the outside of the same thing. But a dualism of attributes or aspects is unintelligible until we ground them in the 'one' reality; and then the one reality

ceases to be one, and we have dualism in a worse form than before. Of genuine monisms the author mentions three: (1) Materialism. But thought cannot be scientifically classed as a secretion of the brain. Materialism, moreover, contradicts the epistemologically fundamental fact that we are given in experience only sensations and ideas derived from sensations. (2) Spiritualism regards material processes as functions of the psychic; and 'on the vanity of such a view we need waste no words.' (3) Idealism accepts the opposition of the material and the psychic, but goes on to ask the critical question whether both are given to us in experience as primary. To this idealism answers, no; and with reason; there stands the fundamental fact of epistemology. Kant tried to overthrow the idealism of Berkeley, but failed. He had rightly limited the knowledge of causal relations to phenomena, but himself fell foul of this limit when he said that we know of a cause for phenomena—a thing-in-itself. We are forced to remain in the psychic, and for this reason the idealistic theory has been called the 'immanental philosophy.' Above all, it is necessary to rid ourselves of the prejudice that our sensations are *in* our brains—what Avenarius called the introjection hypothesis. Sensations by no means have a spatial locus in the brain. "The only place of our ideas is yonder in the world." The world of sensation is to-day reduced by science to movements of mass and the ether, and there are indications that the ether may, *in futuro*, be regarded as a sort of mass. But this 'mass' is certainly not a 'matter' which is different from our sensations. It is impossible to form an idea of anything that is not psychic. Impenetrability and extension, the two most general characteristics of matter, are simply very general marks of our sensations, very general notions of sensations themselves.

But, we add, the place of our sensations is no more 'draussen in der Welt' than it is here in the brain-cells. The difference between yonder and here is an experience which the immanental philosophy must account for—and that, perhaps, by turning to social intercourse and to the social consciousness.

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#### SOCIOLOGICAL.

*Games, Sports and Amusements.* WALTER E. ROTH. North Queensland Ethnography Bulletin, No. 4. Brisbane, 1902.

Mr. Roth gives an exceedingly interesting account of the play activities of the aborigines of northern Australia. The different games



are classified by him as follows: imaginative, realistic, imitative, discriminative, disputative, propulsive and exultative. The classification is, however, as the author states, merely tentative; the importance of the article lies rather in its first-hand description of these aboriginal plays and games. Many of the activities described as play are only such from a conventional civilized standpoint; from the standpoint of the aborigines they are adult serious occupations, ceremonial observances, etc. That which is a matter of religious creed to them may appear to us as legends and stories. The mimicking of animals in a dance is to them a serious part of the hunt; we, on the other hand, consider it as a part of the general propædæutical function of play.

The social values of their jousts or tournaments, such as the opportunity afforded for wiping off old scores and thus settling old disputes, for the exhibition and development of prowess and courage, for co-operation, etc., are excellently portrayed. The *Rausch* cultivated at their ceremonials, by the chewing of leaves of the 'stinging-tree' (*Laportea* sp.), the excessive obscenity of many of their plays, the naïve animism of their legends, the social functions of the 'corroborees,' these and many other good points make the perusal of this bulletin very interesting to a psychologist.

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*Die Wette.* RICHARD M. MEYER. Archiv für Kulturgeschichte, Bd. I., Heft I., 1903, pp. 1-17.

The bet or wager is usually regarded as a sort of struggle, fight or war, and it may without doubt be correctly called such, since it signifies the measuring of strength in a contest for a prize. The wager is, however, distinguished from the other forms of the struggle by five fundamental differentia:

I. The wager as a general rule is distinguished from other forms of fight by the fact that no effort or exertion to influence the outcome or result of the bet on the part of the participants is presupposed. This is obviously true of what the author calls determination wagers (*Feststellungswetten*), where the wager concerns a date or an historical name. This does not hold, however, of futurity wagers (*Erwartungswetten*), as *e. g.*, a wager as to whether an engagement will take place between two young people or as to whether such and such a candidate for office will be elected. It is obvious that a special code of honor thus surrounds the wager, one which has grown up since early primitive times. It is almost wholly a matter of sport in modern times, whatever its possible origin in grimmer days.

II. The wager is a struggle with equal stakes, deposits or pledges. The size or amount of the stakes as deposited by each participant may bear a varying ratio to the stakes of the other participant in accordance with the degree of assurance and certainty in his own mind. In chess or checkers a skilled player may assume a handicap voluntarily. The betting at horse racing involves very complicated rules of observances, judges, holders of the stakes, securities, etc. The deposits or stakes may possibly take the place of the war hostages of former times.

III. The first differential characteristic, that of passivity, led clearly to the second, that of equal stakes. The third follows as a natural consequence that the contest is to be decided by a third higher instance. A weather wager, for example, depends on factors other than the contestants. The oath, a vow, is so to speak a one-sided wager.

IV. The wager is in nearly every case a measuring or pitting against each other of *mental abilities*. It may be of memory, of anticipation and prophecy (in the primitive sense of the term, especially), of comparison. Even in its most material form — *jene greulichsten Fress- und Saufwetten* — the question is really as to which of the contestants has more correctly estimated his own barrel-like capacity.

V. The wager is a struggle ensuing upon mutual agreement or stipulation. Arbitration takes the place of a free-for-all or catch-as-catch-can fight.

The close connection of the wager with play is discussed, as also some of its early primitive forms.

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#### EXPERIMENTAL.

*Discrimination of Shades of Gray for Different Intervals of Time.* FRANK ANGELL. *Philosophische Studien*, XIX., pp. 1-22.

The immediate purpose of the experimental investigation reported in this article was to discover the effect of a lapse of time between the appearance of the norm and the comparative stimuli on the discrimination of different shades of gray.

The experiments were made with intervals of 5, 15, 30 and 60 seconds elapsing between the appearance of the normal and the comparison, and under three different conditions: (1) "Eyes closed during the interval, whilst an effort was made to hold fast a visual image

of the disc. (2) Eyes open with relaxed attention during the time interval." (3) With some distraction introduced during the interval.

Two series of experiments were made, one at Würzburg, the other at Leland Stanford University, the personnel of the observers being changed in the two cases. The results in both series show that correctness in the discrimination of differences is practically independent of the lapse of time between the two stimuli. The percentage of right judgments made with the attention held on the visual image, varies very slightly from that made when the attention was relaxed, as is also the case with those judgments before which some distracting factor was introduced.

Remarks based on the introspection of the observers, from which some explanation of the results is sought, indicate that the visual image of the norm is a comparatively unimportant factor in the judgment, but verbal associations play an important part. From this the writer infers that 'most of the judgments are based on contiguous association, specially on verbal reproduction.' This basis of the formation of the judgment would naturally account for the fact that no marked effect on the correctness of the judgment is caused either by the length of the interval, or the distraction introduced between the appearance of the norm and the comparison. There were also many 'free' judgments, in which no conscious comparison with the norm occurred, but in which the comparative stimulus appeared immediately darker or lighter than any that had yet been seen.

An attempt to secure further data for the explanation of the results was made in the last series by recording the time occupied by the judgment process. This shows, in general, that 'sure' judgments are formed most rapidly, 'like' most slowly, and 'fairly sure' judgments hold an intermediate position.

Concluding, the writer offers three grounds for the explanation of the above and similar results:

1. "From the presence of contiguous reproduction, usually verbal, coming from the formation of a scale of values.
2. "From the presence of free judgments, resulting also from the formation of a scale of values.
3. "From the relatively large number of judgments, 'like' for the shorter intervals, resulting from the maintenance of common conditions during the periods of exposure of norm and comparison."

The work has the marks of resourcefulness and scientific accuracy; and the careful gathering and estimating of remarks based on the introspection of such 'careful and well-trained' observers as Professor

Külpe and the writer himself, make the contribution both reliable and valuable.

*The Time of Perception as a Measure of Differences in Intensity.*

J. McKEEN CATTELL. *Philosophische Studien*, XIX., pp. 63-69.

Professor Cattell in a very few pages opens up some interesting problems and presents a number of exceedingly suggestive and interesting considerations.

After a brief résumé of criticisms of the generally employed psychophysical measurement methods, advanced in an earlier article (cf. *American Journal of Psychology*, Vol. V., pp. 285-294), he proposes a new method of approaching the problem of measurement of the intensity of sensation, which is based on the measurement of the time occupied in perceiving a difference between two sensation intensities or qualities. The shade of gray which takes as long to discriminate from white as from black should, according to this scheme, be the gray which for consciousness is midway between white and black. The method may also be used for testing differences of sensibility.

The experimental sections of the paper include first, a series of investigations in the discrimination of light intensities, carried out by means of a series of 211 shades of gray papers, using the method of average errors. The results show a decrease in the sensibility of discrimination, with the increase of the intensity of the stimulus, not however such a decrease as would satisfy Weber's Law, but rather a decrease approximately in direct proportion to the increase of the square root of the stimulus. This is in accord with the hypothesis proposed in the above-mentioned article, viz., "The error of observation tends to increase with the square root of the magnitude, the increase being subject to variation whose amount and cause must be determined for each special case."

The second section of the experiments investigates the differences in intensity as measured by the time of perception. The same gray cards were used. The results indicate that with the decrease of the difference between the intensities compared, a longer time is required to make the discrimination. This tendency is approximately uniform with the two observers, but as the writer himself remarks, the number of the experiments is quite inadequate to enable one to draw any very definite conclusions.

F. S. WRINCH.

PRINCETON UNIVERSITY.

*Ueber die Beziehungen zwischen Ermüdung, Raumsinn der Haut, und Muskelleistung.* THADDEUS L. BOLTON. Psychologische Arbeiten, Bd. IV. (1902), Heft 2, pp. 175-234.

Professor Bolton has undertaken three series of experiments on the relation of mental fatigue to bodily condition; in all three the æsthesiometer was used to test the validity of Griesbach's results; and in the last two the ergograph was also used, following out the work of Mosso and Kemsies.

In the first series of experiments, fatigue was produced by the addition of numbers from 1 to 100 for different periods:  $\frac{1}{2}$  hour, 1 hour and 2 hours. The tests with the æsthesiometer were made before and after each period and were made on the skin of the forehead. The results showed many variations in the threshold, but on the whole no definite increase of space threshold could be proved as the fatigue advanced.

The second series of experiments with the æsthesiometer led to similar negative conclusions. The ergograph used by Professor Bolton was constructed according to Kraepelin's plan and differed somewhat from Mosso's. The essential modification consists in a contrivance for holding the weight during the period of relaxation of the finger. This holding of the weight prevents the strain which Mosso's ergograph always imposes on the finger and thus reduces the work which the finger ordinarily does during the period of relaxation. The ergograph tests in the second series showed that in addition to mental fatigue, there were many other factors which deserve great emphasis in working out the results. Such factors are: the influence of practice, familiarity with the instrument, and the temperamental disposition of the subject.

Professor Bolton then investigates more fully the relative influences of fatigue and practice, by comparing the results of the adding for successive quarter-hour periods. He finds that the effects of practice overbalance those of fatigue at first, but that as the experiment is continued the fatigue becomes relatively more important, and the effects of practice diminish in strength. Other minor influences enter in, such as the stimulus which the subject feels when he is nearing the end of the experiment (Schlussantrieb).

The third series of experiments reported in the paper verifies the conclusions reached in the first two series.

Professor Bolton's conclusions are essentially negative, both in regard to the æsthesiometric and ergographic tests, and in this they agree with those of other recent investigators. The experiments were



obviously conducted with great accuracy in detail, and conclusions are drawn with due caution. The general problem of the nature of fatigue has certainly not been solved by these tests, but the complexity of fatigue states has been fully demonstrated.

G. B. LOVELL.

YALE UNIVERSITY.

*Ein neuer Fallapparat zur Kontrolle des Chronoskops.* HERM. EBBINGHAUS. *Zeitschrift für Psychologie und Physiologie der Sinnesorgane*, Bd. 30, Heft 4 (1902), pp. 292-305.

This apparatus for the control of the Hipp chronoscope is a substitute for the fall hammer and utilizes a freely falling ball of 27 mm. diameter and 90 g. weight.

On a solid base of wood are erected two nickel bronze uprights 3 cm. in diameter and 86 cm. in height. These uprights are joined by an adjustable bridge which slides up and down and is held on each side at the same height by set-screws. On each upright is a millimeter scale so that the height of the bridge above the base can be read at a glance. In the center of the bridge is a circular opening. On the left end of the bridge is the contrivance for holding and releasing the ball. This releaser consists of a pair of straight brass strips one of which is on either side of the ball. When in position, the strips are exactly parallel and hold the ball securely just over the circular opening in the bridge. The separating of these arms is accomplished by a hand-lever arranged just behind them near the upright. The ball immediately begins its descent when the lever separates these arms. The two parts of this holder are insulated from one another and from all other parts of the machine. An electric current, accordingly, can pass from one to the other only as long as they hold between them the ball. As soon as they spring apart the circuit is broken. The break comes, therefore, exactly at the instant when the weight is released and begins to fall.

In order to place the ball squarely in the releaser, a small plate is attached to the right end of the bridge. This can be lowered or raised on a spring holder and can be turned into the circular opening just under the releaser. The ball is placed on this plate and is kept in position by a small ring. The arms of the holder are closed without moving the ball. The plate is lowered and withdrawn. The ball is now in position over the circular opening.

Being released, the ball drops and strikes below upon one end of a board which is nicely balanced at its center. The weight of the ball

lowers the end on which it strikes and raises the other end. The instant the end is raised a second electric circuit is broken. Thus, without any intermediate interruption, the ball controls the two breaks of the electric current, one at the beginning and one at the end of the fall.

The apparatus was tested for various heights to ascertain whether the times marked off by the two broken contacts agreed with the time theoretically demanded. Plates are given showing comparisons of the apparatus with a standardized fork. The results show that the error is well within a single sigma.

Details are given showing various series of connections which make it possible to utilize two breaks in making records or in testing the Hipp chronoscope.

W. M. STEELE.

YALE UNIVERSITY.

*Minor Investigations in Sense Perception.* R. MACDOUGALL. *Am. Journ. Psychol.*, Vol. XIII., No. 4.

These are certain minor investigations undertaken in connection with the problem 'concerning the subjective determination of the primary point of regard,' reported in the 'Harvard Psychological Studies' (Monograph Suppl. *PSY. REV.*, No. 17).

The first (I.), 'On Determinations of the Subjective Horizon by Motor Coördination,' is an attempt 'to ascertain the relation of the subjective horizon of the eye as determined by raising the index finger, to its position when determined visually, and the influence upon such location of changes in the orientation of the head and eyes.' The displacement is much less in the case of the downward movement of the eyes and head than with the upward movements. "The upward rotation of the eyes in their sockets develops a relatively intense strain experience, while in rotations of equal magnitude downward from the primary position of the eyes these muscular tensions are practically lacking. This difference arises from the biological relations of the organism to its environment, which call forth constant exploring movements of the eyes within the lower half of the field of vision, while very few are made above the horizon in the expanse of the sky." That is, 'these forms of spatial orientation are related to oculo-motor conditions, and the direction of the characteristic errors which they present are dependent upon the coördination of eye and hand in the perception reactions of ordinary practical life.'

The second (II.) is on 'The Relation of Saturation in Homogeneous Colors to the Area over which the Color is Spread.' "When the

whole hand is plunged into warm water, for instance, it feels hotter than when only the tip of the finger is immersed." The same is true of taste solutions. "It is a natural inference from the connection which is found in these instances that the number of elements of the sensitive surface stimulated and the intensity of the resulting sensation stand always in such a relation of functional dependence that the subjective estimation of the intensity of a sensorial stimulus cannot be considered apart from the magnitude of the area excited." "In the case of certain senses it has been noted further that this summation effect is independent of continuity in the surface to which the stimulus is applied; intensive reinforcement takes place when the sensitive elements affected are not contiguous but form a discrete series." Thus in color vision 'there is least difference in saturation between small and large areas of red, of all the colors observed, and most difference in the case of green,' with blue, yellow, violet, orange in order between. "Therefore, the influence of the number of elements stimulated upon the intensity of the color sensation is greatest in the case of green, least in that of red."

The third investigation (III.) is a brief examination of the 'Quantitative Relations of Stimulation Area and Color Threshold in Discrete as Compared with Continuous Extents.'

H. HEATH BAWDEN.

VASSAR COLLEGE.

*Ueber Hemmung gleichzeitiger Reizwirkungen.* PAUL RANSCHBURG. Zeit. f. Physiologie u. Psychologie d. Sinnesorgane, Band XXX.; Heft 1 u. 2.

This is an experimental contribution to the theory of the conditions of attention. The work was devoted to the determination of the nature and explanations of certain errors that had appeared in some previous investigations upon the correctness of the capacity for comprehension (*Sicherheit der Auffassungsfähigkeit*). The test was made of the capacity to reproduce correctly all the digits in a six-place number when they were passed before a slit in a card on a revolving drum. An exposure of one third of a second was allowed. The analysis of the results showed that the false reading of a number appeared proportionately more frequent in these numbers where, in the four places at the right hand, particularly in the third to the fifth place, two identical or two similar figures next to one another or separated by one or two figures were found, whereby the identity as well as the similarity of the several elements appeared considerably more impor-

tant. The remainder of the research turned upon the value of identical and similar elements among a number of successively presented figures. Numbers that contained repeated figures proved more difficult to grasp than those that contained no repetitions. The proposition with which the experimenter sets out is this: "The threshold for comprehension of simultaneous or quickly successive heterogeneous stimuli lies deeper than that for homogeneous stimuli." Series of six-place numbers were constructed upon the basis of repeated and entirely different digits and called respectively homogeneous and heterogeneous series.

The number of errors for the heterogeneous series is about one third that for the homogeneous. For the latter there were 108 errors in a possible 180. The character of the errors is very different for the two classes of stimuli. Inversions of the order of two figures are not only greater proportionately, but really, for the heterogeneous stimuli. The larger proportion of the errors for the homogeneous series are either substitutions of one digit for another or blanks from which a digit has been dropped. The errors of whatever character in the homogeneous series usually occupy one place only while they are two-place in the heterogeneous series. Similar digits seem to act in this inhibitory manner much the same as identical or repeated digits. Similarity of digits was allowed to exist when any two digits were frequently interchanged for one another. The inhibitory effect of various similarities was of different degrees, depending upon the degree of similarity. Zero was found to have a higher heterogeneous value than any of the other nine digits. The subjective declarations of the reagents, although they knew nothing of the homogeneous and heterogeneous character of the series, show that they found themselves more often in uncertainty with respect to the series containing identical elements, although uncertainty did not always mean false comprehension. The author holds that all the phenomena become intelligible upon the supposition of the retarding influence of inhibition—'in a short space of time that is just sufficient for the sharp up-building of two psychological processes of a heterogeneous nature, two processes of a similar nature cannot be conceived as autonomous processes separate from one another, as a result of which the analyzing consciousness receives the impression of one process only, the more identical the two processes were.' This supposition 'that simultaneous homogeneous stimuli-effects inhibit one another in their development and lead in the psychological field to the apparent blending of simultaneous homogeneous sensations' is shown to be applicable to every form of sensation

process. The paper presents an exceptionally interesting and valuable piece of work along lines that have not received the attention they deserve.

THADDEUS L. BOLTON.

UNIVERSITY OF NEBRASKA.

*An Experimental Study of Writing Movements.* CHARLES H. JUDD. Philos. Studien, XIX. (Wundt's Festschrift, I.,) pp. 243-259.

This study aims to discover the relation of consciousness to the acquirement of the writing movements, and how this relation changes as the movements become automatic. The apparatus used was a brass spring clasped tightly around the fifth metacarpal bone just behind the little finger, and to which was attached a short aluminum rod carrying at its outer extremity a glass tube held in a vertical position. A writing stylus passed through the tube was allowed to write through the pressure of its own weight. The same hand of the subject wrote with a pen held in the usual way. As the stylus wrote with only the movements of the hand, and the pen with the combined movements of the hand and fingers, a comparison between the two writings shows how much is done with the hand and how much with the fingers.

It was found that the fingers do the work of constructing the letters, while the hand participates only in the forward movements, and the arm acts in the intervals of the words to carry the hand forward. In learning to write one is unconscious of the kind of movements he makes, but gropes about until an easy and effective set of movements is acquired which makes the result conform to the visual pattern. If this pattern is tenaciously held to, the writer may come to produce a perfect copy, but in so doing he sacrifices mental content to form, since attention to form crowds out attention to content. Herein lies the key to individuality in handwriting. If the visual pattern is relinquished early, individual variation is more pronounced, because attention is relieved from its control.

While the study makes an interesting beginning, one regrets that it could not have been carried farther and embodied more results. A real science of chirography is perhaps attainable in this direction.

JOHN P. HYLAN.

CAMBRIDGE, MASS.



## ILLUSION.

*Zur Lehre von den Urtheilstäuschungen.* O. ROSENBACH. Zeitschrift f. Psychol. u. Physiol. der Sinnesorgane, XXIX., 443-448.

The observations upon which the theoretical parts of this paper are based are as follows: Cut out small geometrical patterns from colored paper; and place across these patterns, in such a way as to cover up their middle parts, a strip of wholly untransparent paper. The covering strip should be about 1 cm. in width, and the figures should be long enough to extend beyond both sides of this strip. When the figures covered by the strip are viewed in a not too strong light, the outlines of the covered parts will seem to appear through the covering strip as if it were transparent. This is explained as due to an unconscious judgment. A variety of familiar cases in which percepts are filled in, such as the case of the blind spot, the case of a misprint, etc., are cited as analogous unconscious judgments.

The paper may be discussed with reference to two distinct questions. First, is the apparent transparency of the covering strip explicable on any hypothesis other than that of unconscious judgment? In dealing with this question the author dismisses in a very summary fashion the possible explanations by irradiation and after images. When we recognize, however, that a weak light is one of the chief conditions for the observation, these explanations based upon retinal conditions seem to call for much more careful consideration.

The second question open to discussion is the much-debated question of whether one is justified in using the concept judgment in such a connection. The paper does not help to advance this discussion, for it simply assumes the unconscious judgment and does very little, if anything, to define or defend the assumption.

CHARLES H. JUDD.

YALE UNIVERSITY.

## FEELING.

*Sur le senil de la vie affective.* GASTON RAGEOT. Revue Philosophique, February, 1903, Vol. LV., pp. 153-175.

This article is an attempt to solve the question of the nature of the affective life, on the basis of the author's observation of the development of the emotional life of several children. In the course of the article recent theories such as those of Lehmann and James are subjected to a critical examination.

In opposition to the theory of Lehmann, the author holds that the earliest conscious life of the child is one of pure feeling. This feeling is at first pain. The relief of pain results in feelings of pleasure,

which soon become the positive accompaniment of unobstructed motor activity. On the other hand it is held that Lange and James go too far in attempting to connect emotions wholly with the exercise of activities, rather than with the organization of activities.

Throughout the article the author lays emphasis upon the organization of activities both mental and physical as the essential condition of emotional life. It is shown that the same physical expression may accompany widely different forms of affective mental life. The movements accompanying colic, for instance, are exactly similar to those which accompany anger. The manifestations usually expressive of a certain emotion may also become transferred to another through association. These facts show that the emotion is dependent upon the way in which the organization has taken place, rather than upon the purely physical activity accompanying it.

Three stages of emotional life and corresponding development of organization are recognized. The simplest emotions which are the only kind existing for the very young child occur when organic movements, originally reflex, become the expression of a process of organization and disorganization, called forth by one or more motor images.

A second kind of emotion begins to appear in the child after the sixth month. This is of a less violent type than the first. Here the sensory-motor associations of custom and experience begin to realize themselves under the control of familiar images. This is the type of the ordinary emotion.

Finally a stage is reached in which, though the motor images are present, the movements are not actually executed, but remain as mere tendencies to action. Thus, as we ascend the scale of emotional development, it appears more clearly that the actual execution of the movement is not an essential condition of the emotion. On the other hand it is obvious that in all cases the condition of emotions is to be found in the organization which either reinforces or interferes with tendencies towards activities.

YALE UNIVERSITY.

E. H. CAMERON.

#### ATTENTION AND CONSCIOUSNESS.

*Fluctuations of Attention and After-images.* EDWARD A. PACE.  
Philos. Studien, XX. (Wundt's Festschrift, II.), pp. 232-245.

We may assume that fluctuations are produced centrally, in which case the conditions in the sense organ do not change, or that changes take place simultaneously in the sense organ and the brain. The present study aims to throw some light on the part the retina of the eye plays in producing the phenomenon.

A semi-transparent porcelain plaque was fixed in the side of a box, between which and a light placed on the inside was a plate of ground glass lined with paper and a sheet of cardboard having a horizontal slit for letting the light through. In front on the outside was another light made adjustable for varying the illumination of the visual field. A movable screen held in position by an electro-magnet was used for cutting off the light from within the box when desired. The subject fixated the band of light, and as it disappeared allowed the screen to fall and cut off the stimulus. In this case the after-image appears, but if the fluctuation takes its course until the moment of reappearance before the screen is dropped, no after-image, or only a barely perceptible one, is seen. There thus appears to be considerable fatigue present in the retina at the time of disappearance but practically none at the reappearance. From this it appears that from the point of appearance to that of disappearance there is a phase of increasing fatigue, and from the point of disappearance to that of appearance is one of decreasing fatigue.

In spite of the several studies which have been published upon this subject, the development is slow and much the same ground has continually to be plowed over for a preliminary discussion. A fundamental advantage would be gained by having a basis of distinction between the fluctuations peripherally and those centrally originated. Such a one may be found in the fact that when the peripherally conditioned disappearance has taken place, the mental image may yet be retained. Although the direction of attention may be slightly different when fixed on an external object than when on the remembered image of it, there is yet a sufficient similarity between the two states to prevent there being a fluctuation of the attention properly speaking. What, therefore, has most commonly been called a fluctuation of attention is essentially a fluctuation in the functioning of the sense organ, or possibly in the sensory tract before the ideational centers are reached.

J. P. HYLAN.

CAMBRIDGE, MASS.

*Zur Theorie des Bewusstseinsumfanges und seiner Messung.* WILHELM WIRTH. Philos. Studien, XX. (Wundt's Festschrift, II.), pp. 487-669.

Of this contribution one hundred and forty-eight pages are devoted to theoretical discussion, nineteen to the description of new apparatus, and eleven to experimental results. There have been several studies of late either directly or bordering upon the question of the boundaries

of consciousness, and the present discussion aims to clear the ground for more intelligent work.

The starting point is the well-known experiments of Wundt with the tachistoscope, and that in which two consecutive series of metronome strokes are compared to find how long these series can be and yet have their relative lengths rightly judged. The question arises as to why in the former experiment only from four to six objects can be perceived from an instantaneous exposure, while in the latter from sixteen to forty may be correctly compared. A cue is found in Cattell's work, where it was observed that in addition to the four or five simple objects that could be clearly seen, there was an estimation of the whole number, but characterized by a large average error and a tendency to underestimate. The greater distinctness and smaller number of objects as compared with the auditory experiment is the result of greater practice in seeing these particular forms. Also with the series of metronome strokes there is a tendency towards a rhythmical grouping which corresponds to the grouping of letters into words with the visual impressions and which make it possible to see a much larger number of letters. In both experiments no judgment is possible until the sensory impressions are passed, and in both two images are compared; in the one case the two series of strokes, and in the other the memory of the visual impression with the expressed judgment. If an arrangement of the tachistoscopic experiment could be made to conform more closely with the conditions of the auditory experiment, more similar results could be expected.

To this end a new form of tachistoscope was constructed, with which it was possible to present to the subject two groups of simple figures for the purpose of comparison, one quickly following the other. In the main the two groups were alike, but with one or more changes of the figures in the second. It was found that changes could be indicated correctly within a limited region of the point of fixation. When twenty-five figures made up the group, not more than one change in the outer edge would be noted if the attention were not directed there. Whenever there were several changes, only one would be noted, except when they were close together.

The author is well informed and the discussion well arranged. A condensation of the theoretical part would greatly increase its value, although an 'exhaustive' discussion, as with Kant, may derive much of its value by exhausting the reader to the point of consistently avoiding the past of a problem in favor of its future. The apparatus described is ingenious and somewhat elaborate. We trust that a more

extensive contribution to experimental results will come from it in the near future.

J. P. HYLAN.

CAMBRIDGE, MASS.

### NEW BOOKS.

- Genetic Psychology for Teachers.* C. H. JUDD. New York, Appletons. 1903. Pp. xii + 329.
- A Survey of English Ethics: being the First Chapter of Mr. Lecky's History of European Morals.* Ed. by W. A. HIRST. London and New York, Longmans. 1903. Pp. li + 180.
- La Science et l'Hypothèse.* H. POINCARÉ. Paris, Flammarion (no date). Pp. 284. 3.50 fr.
- The Study of Mental Science.* J. BROUGH. London and New York, Longmans. 1903. Pp. 129.
- Experimental Psychology and Culture.* G. M. STRATTON. New York and London. 1903. Pp. viii + 331. \$2.
- Reports of the Cambridge Anthropological Expedition to Torres Straits.* Vol. II., Pt. II. *Hearing.* CHARLES S. MYERS. Cambridge. 1903. Pp. 142-223.
- Aristote.* C. PIAT. Paris, Alcan. 1903. Pp. viii + 396. 5 Fr.
- La Meditazione.* G. A. COLOZZA. Naples, L. Pierro. 1903. Pp. 311. L. 3.
- Why the Mind has a Body.* C. A. STRONG. New York and London. 1903. Pp. x + 355.
- Spinoza's Political and Ethical Philosophy.* R. A. DUFF. Glasgow, Maclehose. 1903. Pp. xii + 519.
- More Letters of Charles Darwin.* 2 vols. Ed. by FRANCIS DARWIN and A. C. SEWARD. New York, Appletons. 1903. Pp. xxiv + 494, and viii + 508.
- History of Philosophy.* W. TURNER. Boston and London, Ginn. 1903. Pp. x + 674.
- Contemporary Psychology.* G. VILLA. Trans. by H. MANACORDA. London, Sonnenschein; New York, Macmillans. 1903. Pp. xv + 396. \$2.75.
- Gesammelte Abhandlungen zur physiologischen Optik.* A. KÖNIG. Preface by TH. W. ENGELMANN. Leipzig, Barth. 1903. (A collection of thirty-two papers.)



- L'Imagination.* L. DUGAS. Paris, Doin. 1903. Bibl. de Psychologie expér. Pp. 350. 4 fr.
- L'Image Mentale (Evolution et Dissolution).* J. PHILIPPE. Paris, Alcan. 1903. Pp. 151. 2.50 fr.
- Le Sentiment religieux en France.* L. ARRÉAT. Paris, Alcan. 1903. Pp. 158. 2.50 fr.
- Le Mensonge.* G. L. DUPRAT. Paris, Alcan. 1903. Pp. 190. 2.50 fr.
- Essai de Classification naturelle des Caractères.* CH. RIBERG. Paris, Alcan. 1903. Pp. xxiv + 199. 3.75 fr.
- Primer on Teaching.* J. ADAMS. Edinburgh, Clark. 1903. 6 d.
- Zur Atombewegung.* J. HUNDHAUSEN. Leipzig, Barth. 1903. Pp. 54. Mk. 1.20.
- Zur Grundlegung der Psychologie des Urteils.* E. SCHRADER. Leipzig, Barth. 1903. Pp. 98. Mk. 3.
- Studies on the Psychology of Sex.* H. ELLIS. Philadelphia, Ellis. 1903. (First Section of Vol. III.) Pp. vii + 55.
- Spelling in the Elementary School.* O. P. CORNMAN. Exper. Studies, ed. by L. Witmer, No. 1. Boston, Ginn. 1902. Pp. 98.
- The Sensation of Pain and the Theory of the Specific Sense Energies.* ANNA J. MACKEAG. (Same Series, No. 2. Boston, Ginn. 1902.
- Psychological Norms in Men and Women.* HELEN B. THOMPSON. Univ. of Chicago Cont. to Philos., iv, i. Chicago, Univ. Press. 1903. Pp. 188.
- Certain Aspects of Educational Progress.* Various Authors. Invest. Dept. of Psych. and Ed., Univ. of Colorado. 1903. Pp. 84.
- Outlines of Psychology.* JOSIAH ROYCE. New York and London, Macmillans. 1903. Pp. xxvii + 392.
- L'Ennui.* E. TARDIEU. Paris, Alcan. 1903. Pp. 297. 5 fr.
- Introduction to Philosophy.* W. T. MARVIN. New York and London, Columbia Univ. Press, Macmillans. 1903. Pp. xiv + 572.

## NOTES.

WE note the appointments: Dr. B. Bosanquet and Dr. G. F. Stout to the two philosophical chairs vacant in the University of St. Andrew's; Dr. Montague of the University of California, to an Instructorship in Philosophy in Columbia University; Dr. Wrinch of Princeton to an Instructorship in Psychology in the University of California. Professor William Caldwell, of Northwestern University, and Mr. A. E. Taylor, of Owens College, Manchester, respectively to the moral and mental philosophy chairs in McGill University, Montreal; and Mr. Carveth Reid to the Grote Professorship of Philosophy of Mind and Logic in University College, London.

PROFESSOR JAMES R. ANGELL is lecturing on psychology in the summer session of the University of California.

THE prospectus has been issued for a new Journal of Psychology to be issued by a board of editors include Professor James Ward and Dr. Rivers of Cambridge University. The journal is to publish researches and original papers and will appear at irregular intervals. Further announcement will be made later on.

ANOTHER new serial publication, also to appear irregularly, is *Beiträge zur Psychologie der Aussage*, of which the first number has reached us. It is issued from the press of Barth of Leipzig and is edited by Dr. L. William Stern with the coöperation of a weighty committee. (Price 4 Marks per number.)

